Chapter One
Chronological Foundations

19th Dynasty Chronology

In our previous book we set in place the chronologies of the 20th, 21st, and 22nd dynasties, arguing that the three dynastic groups set foot in Egypt around the year 760 B.C., as part of the forced migration of multiple nations bording on the Mediterranean, nations referred to in the traditional history as the "Sea Peoples". We promised that we would, in this book, lay the groundwork for that event, explaining its probable cause, and establishing its dates with some degree of certainty. Our first task in this endeavor is to assign provisional dates to the 19th dynasty kings, particularly to Ramses II and Merenptah.

19th Dynasty Chronology

In the traditional history the 19th dynasty ended in a period of civil unrest in which four or five of the immediate descendants of Ramses II vied for power. There is even conjectured a brief interregnum at the end of the 19th dynasty, based on a solitary (but reliable) historical reference in the *Harris Papyrus*, a document we will examine momentarily. The end of the 19th dynasty is also associated with at least one major physical catastrophe, which contributed both to the downfall of the famed Anatolian Hittite Empire and the end of the Canaanite culture which flourished in the city state of Ugarit. These were troublesome times, from which emerged the 20th dynasty under its patriarch Setnakht, the father of Ramses III.

This is not the time to examine the details of this physical and social upheaval. Needless to say that, according to our reconstruction of history, these events must all be dated to the middle years of the 8th century B.C., and that there emerged from the ashes of the 19th dynasty not one, but three Egyptian dynasties, numbered 20 through 22 in Manetho's numeration schema. Our interest in this chapter lies strictly in the chronology of the times.

The traditional history, as represented by Alan Gardiner in his epic *Egypt of the Pharaohs*, assigns to Ramses II and his son and successor Merenptah the years 1290-1224 B.C. and 1224-1214 B.C. respectively, while Setnakht is dated in the years 1184-1182 B.C. In the thirty years which lie between Merenptah and Setnakht (1214-1184 B.C.) is placed the internecine feuding among those vying to succeed Merenptah, including Sethos II, Amenmesse, Siptah and Twosre. This chaotic interlude also included the machinations of the "king maker" Bay. Since Twosre, the last of the interlopers, is assigned the years 1202-1194 B.C. by Gardiner, there is clearly assumed an interregnum of ten years between Twosre and Setnakht. All of these dates, however, are pure conjecture. Most of the (few) extant monuments from this time frame are undated and there is no clear evidence that these kings ruled in succession to one another. In fact, the evidence suggests otherwise.

We believe that the interval between Merenptah and Setnakht was much shorter than that suggested by Gardiner, possible as little as five years. As we will soon see, Merenptah's activity was confined to the north of Egypt. The strivings of Sethos II and the other named contestants for power seem to be restricted to the vicinity of Thebes. We believe they overlapped to a large extent the rule of Merenptah in the delta. And we cannot fathom any reason for assuming an interregnum longer than a few years. Accordingly we reduce the interval between the 10th year of Merenptah and the 1st year of Setnakht from thirty years to five. And since in book two we dated the first year of Setnakht to the year 759 B.C., not the 1184 B.C. of the traditional history, we must reduce the dates of Ramses II and Merenptah to 840-774 B.C. and 774-764 B.C. respectively, a 450 year reduction from the traditional dates. Needless to say, if we are correct, the famed warrior king Ramses II could not possibly be the pharaoh of the Exodus, as is typically claimed, no matter how much scholars lower the dates for that event. He was a contemporary of Jeroboam II of Israel. Moses was not a contemporary of Ramses II as proponents of a late date Exodus maintain. He died over seven hundred years before Ramses began his lengthy rule in Egypt.

The balance of this book will be devoted to proving either the relative or the absolute chronologies herein proposed. In particular we need to justify our moving the 21st dynasty of Theban priest/kings forward in time

over four hundred years to overlap the latter part of the 22nd dynasty. We also need conclusive evidence that the 20th, 21st, and 22nd dynasties all followed, almost immediately, the 19th dynasty rule of Ramses II.

The balance of this chapter presents three broad strands of evidence that contribute to this goal: 1) the Berlin and Louvre genealogies of the Deltite high priests of Ptah; 2) the sequence of deaths of the sacred bulls of the Osiris/Apis cult from the famed Serapeum near Memphis; and 3) the evidence related to the ordering of the Theban high priests of Amun. We examine them in reverse order.

The Missing High Priests of Amun

To date we have outlined a chronology strictly in terms of the kings of the various dynastic groups. But the clergy were almost as vocal as the kings, and they have left behind a considerable body of literature, some of which contributes significantly to proving our case. In particular the Theban high priests of Amun, at least those few who functioned during the latter half of the 22nd dynasty and beyond, lend support to our cause, not by what they say, but by what they do not say, i.e. by their apparent silence in the literature of the traditional history. While not directly related to 8th century chronology, the mysterious silence of the high priests of Amun during the late 22nd through early 25th dynasties indirectly supports our cause. Clearly a word of explanation is necessary.

When we moved the 106 years of the 21st Theban dynasty forward four hundred years into the 7th century, placing Herihor, Piankh, Pinudjem I, Masaharta, Menkheperre, Pinudjem II and Psebkhannu III as successors to the high priest Takeloth, at precisely the time when this pontiff assumed the kingship and became Takeloth III (Tarqu), there should have resulted some serious conflict with the traditional history. At the time we refrained from asking the question we now propose. But it needs to be asked. On the assumption that the traditional history has no Theban high priests positioned in the 100 year interval which separated the high priest Takeloth (672 B.C.) and the 6th year of Taharka (565 B.C.), where they would clearly conflict with our revised history, we wonder what happened to them?. And while we are pondering that question we take the

opportunity to go further afield and pose a related query. Since we have argued that there existed, following the Babylonian invasion of Egypt in the early years of Taharka, a twenty year interregnum which preceded the advent of Psamtik I and the beginning of the recovery of temple worship, we wonder whether the traditional history has recorded this additional twenty year gap in the high priesthood. Both questions receive the expected response from traditionalist historians. The Theban high priesthood was inexplicably (but conveniently) vacant precisely where it had to be for our thesis to be credible!

According to Kenneth Kitchen

> What happened to the high-priesthood of Amun of Thebes after the accession of Takeloth III *remains a total mystery*. While perhaps some record of a couple of incumbents for the 40 years down into the reign of Shabako remains to be found, yet the simplest explanation may well be that the office was actually left in abeyance. TIP 164 (italics added)

We can disregard Kitchen's figure of 40 years in the above statement. The problem is much larger than this. In his table 13[1] Kitchen admits that the hiatus in the high priesthood lasted at least 50 years. And according to him the silence ended only with the arrival of a high priest named Haremakhet, son of Shabako, who was followed by his son Harkhebi, who was in office in the 9th year of Psamtik I. There then followed *another half-century gap* in the high-priesthood until the arrival of the god's wife Ankhnesneferibre, who also claimed for herself the office of high priest of Amun. All together Kitchen is compelled to leave the high priesthood vacant for a total of 100 plus years, and he achieves this low number only by lowering the dates for Takeloth III, and by assigning to Haremakhet 44 years in office, based on nothing but speculation.

We reproduce below as our table 1 the relevant section of Kitchen's table 13. Note that for the years 775-765 Kitchen records "an unattested pontiff," (equivalent to saying there was no high priest) in the spot where our revised history names as HPA the Prince Osorkon, who later became Osorkon III. Kitchen lists this Osorkon two spots earlier in his table. Clearly he follows a different relative chronology than our revised history.

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[1] TIP p. 480

Table 1: High Priests of Amun from the late 22nd through the 26th dynasty (latter half of Kitchen's table 13)

800-775	Takeloth E – possibly a younger son of Nimlot C
775-765	*An unattested pontiff*?
765-754	Takeloth, son of Osorkon III, who became king Takeloth III
754-704	*Half-century interval*: two unattested pontiffs, or office left vacant
704-660	Haremakhet, son of Shabako (max tenure; could begin later)
660-644	Harkhebi, son of Haremakhet
644-595	*Half-century interval*: two unattested pontiffs, or vacant
595-560	God's wife Ankhnes-Neferibre
560-550?	Nitocris B, daughter of Amasis II

We argue, against Kitchen, that the high priesthoods of Haremakhet and Harkhebi existed back to back in the recovery period which began with the death of Taharka, over twenty years after the 565 B.C. invasion of Nebuchadrezzar. Since Haremakhet is associated with Tanuatamon in several inscriptions, his dates are probably c.a. 543-535 B.C., while those of his son and successor must be roughly 535-510 B.C.[2] We also believe that Nitocris, not long after her "adoption" and at the time of death of Harkhebi, took over the role of high priest at the behest of Psamtik I, and in time handed that office over to Ankhnesneferibre. In the revised history the only vacancy in the Theban high priesthood took place during the first half of the Egyptian exile (564-543 B.C.). The problem of empty pulpits simply does not exist. Egyptologists are searching for what they will never find, high priests to fill a void they have artificially created by moving the 21st Theban dynasty priestly successors of Takeloth III hundreds of years back into the remote past.

Enough said about temporal concerns related to the latter part of our revised chronology. The other two priestly sources speak to issues more directly concerned with our 8th century chronology.

[2]Haremakhet must have been a son of Shabaka's old age, since Shabaka died around 580 B.C. We believe Haremakhet lived in exile in Napata with Taharka and likely moved to Egypt during or immediately following Tanuatamon's invasion in 544/3 B.C.

Missing Apis Bulls of the 21st Dynasty Osiris Cult

Many times already we have had cause to seek the testimony of various stelae found by Mariette in the vaults of the Memphite Serapeum of the Osiris/Apis cult in the latter half of the 19th century. Much of the relative chronology of the late 22nd through 26th dynasties (of both the traditional and revised histories) depends on documentation found on the official stelae deposited by clergy of the cult at this location. What surprised Mariette at the time of his discovery, and has continued to perplex Egyptologists in the century and a half following, is the complete absence of evidence attesting the involvement of priests from the 21st dynasty and the initial years of the 22nd dynasty, a time span of several hundred years. Did the cult cease to exist for this extended length of time?

On the desert surface east and south-east of the ground above the "lesser vaults" Mariette found a series of tombs built individually for the sacred bulls which died in the 18th dynasty under Amenhotep II and his successors. These tombs continued through the reign of Haremheb and into the time of Seti I. The subterranean "lesser vaults" (see Figure 3 below) had their origin under Ramses II, in whose reign at least five vaults were built (Mariette's G - K) with a common corridor. Vault construction was extended northward in succeding generations with the addition of four vaults (L-O), three attributed to Ramses III and his successors, and one left unidentified (inconnu). Though Mariette attempted to associate several of these individual crypts with more than one Apis, there is no evidence in any of these tombs to suggest that fact, other than the presence of dedicatory material in one of the tombs naming multiple 20th dynasty kings. On the assumption that an Apis bull in these times lived an average of around 13 years[3] these four tombs can represent at most around 50 years. Thus, whether we are speaking of the traditional or the revised histories, they can represent only the time interval between Ramses II and the ephemeral successors of Ramses III. In the revised

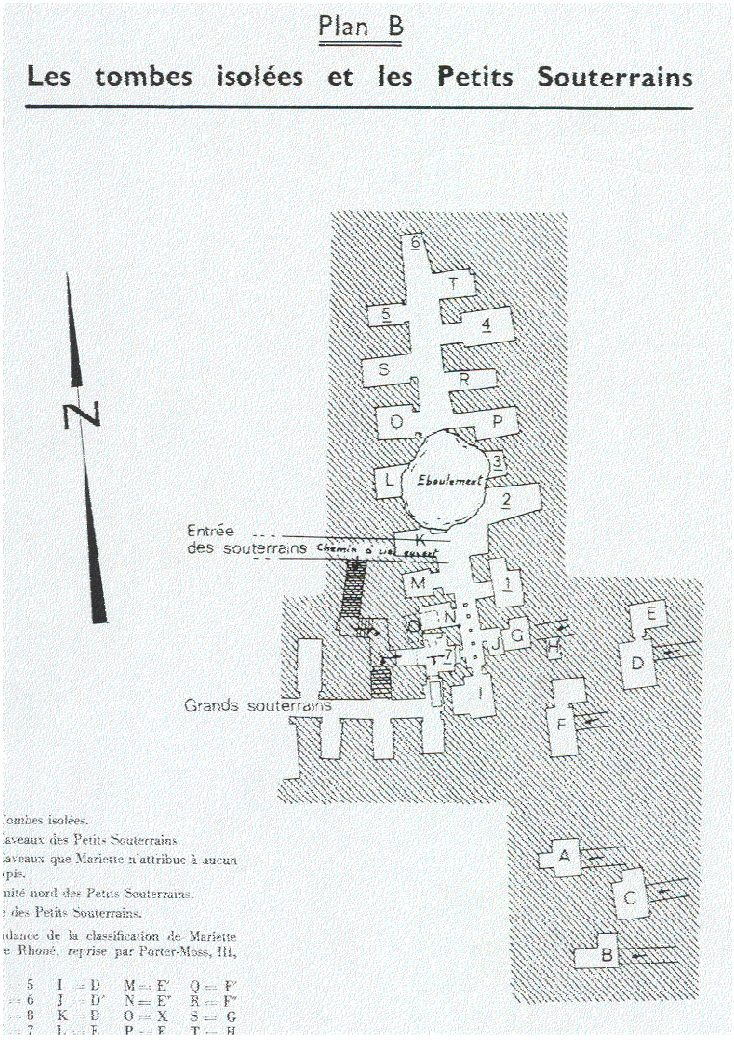
[3]Egyptologists have reckoned that the average life-span of pre-Ptolemaic Apis bulls is around 16/17 years (cf. TIP 126), but we are here dealing with bulls that lived in the midst of twenty years of political and geological upheaval. We do not mean to imply by the dates given that each bull lived 13 years. The probability is that several of these bulls died prematurely resulting from the turbulent conditions which surrounded them. This would lessen the average life span considerably.

history, on the assumption that the first bull died mid way through the reign of Merenptah, the deaths of the four bulls might be dated 768, 755, 742 and 729 B.C. respectively. We are not concerned with the fact that later Ramesside kings might have left dedicatory inscriptions in these vaults; it is the probable sequence of bulls that is important.

After the bulls from the Ramesside era the corridor was gradually extended much further north by the 22nd dynasty kings and their 24th and 25th dynasty successors, though the first bull, that belonging to the 23rd year of Osorkon II (718 B.C.) must have been laid to rest somewhere in the southern complex, where the stela which records the event was discovered by Mariette. These later burials, which ran sequentially from Osorkon II through the early years of Psamtik I were all built off this extended corridor (P-T). The most northerly burial (T) belongs to Psamtik, in his early years, after which a "cave-in" (*eboulement*) over vault L caused Psamtik to begin a new sequence of vaults (the "greater vaults") extending to the west. At least six vaults were built off the corridor in these "lesser vaults" which were apparently intended to be used, but which were left unoccupied because of the cave in (Nos. 1-6).

Egyptologists rightly ask themselves what happened to the bulls that died during the reigns of the 21st and early 22nd dynasty kings. The Serapeum vaults seem to preclude their (independent) existence. But we know what happened. We have already suggested dates for the death of all bulls between Ramses II and the 23rd year of Osorkon. They fit perfectly, leaving no gaps in the sequence. The 21st dynasty kings were contemporaries, first of the early Ramessides and then of the later 22nd dynasty pharaohs. Apparently they were not associated with the cult of Osiris. Their preoccupation was with the god Amun. The early 22nd dynasty kings were contemporaries of the early Ramessides. They were probably confined to the north-western region of the delta and had little association with Memphis. Only with Osorkon II did the seat of power shift to the Bubastis region and the 22nd dynasty become involved with the Apis cult. In the revised history there is no 200 year-long break in the sequence of Apis bulls. All is well with the Osiris cult as it was with the Theban high priests of Amun. The problem lies squarely on the shoulders of the errant chronology of the traditional history.

Figure 1: Mariette's Plan B of the Lesser Vaults of the Serapeum



The Berlin & Louvre Genealogies of the Memphite High Priests of Ptah

The high priests of Amun and of the Osiris/Apis cult are not the only clerics who lend support to our thesis. The high priests of Ptah, listed in

two important genealogies, have a word to add. The most economical means of introducing these genealogies is to quote from Kenneth Kitchen's massive *Third Intermediate Period* which has this to say:

> Two major genealogical documents form the core of our knowledge of Memphite pontiffs for this period, and (combined with contemporary data) have an important and direct bearing on 21st-Dynasty chronology. These are the remarkable genealogies of Memphite priests, one of which is in Berlin (23673) and the other is a partial parallel from the Serapeum, which is now in the Louvre ('96; Cat. 52). (TIP 151)

We have listed in the following chart twelve consecutive generations based on the two genealogical sources.

Table 2: A synchronized genealogy based on the Berlin and Louvre stelae

Named Kings	Priests (Berlin)	Priests (Louvre
2.5 Ramses II	HPM (Karnak), architect	---
2.4 Ramses II	HPM, [---]nashanet(?)	---
2.3 Ramses II	HPM, Ptah-em-akhet	---
2.2 Ramses II	HPM, Neferrenpet	---
2.1 ---	HPM, Ptah-em-akhet	---
1.15 Amenemnisu	HPM, Ashakhet (A)	---
1.14 Akheperre Setepnamun	HPM, P(i)p(i) (A)	7: HPM, Pipi (A)
1.13 Psusennes	HPM, [Har]siese (J)	6-7: HPM, Harsiese (J)
1.12 Psusennes	Prophet, P(i)p(i) (B)	5-6: HPM, Pipi (B)
1.11	Prophet, Asha-khet (B)	5: HPM Asha-khet (B)
1.10	Prophet, Ankhefensekhmet	4: HPM, Ankhefensekhmet
1.9	CS, Prophet, Shedsunefertem (A)	3-4: Shedsunefertem (A)

It almost goes without saying that the two genealogies identify each cleric as the son of the priest in the preceding generation. And since several of these priests are said to have exercised their trade under specific kings, they provide a valuable tool for reconstructing the chronologies of

dynasties 21 through 18, since kings from each of dynasties 18 and 21 are clearly identified. And if we extend the Louvre genealogy downward to its beginning, we can trace the family lineage back to its origins beyond the end of the 22nd dynasty.

The Louvre Serapeum stele was dedicated at the death of an Apis bull by a priest named Ashakhet, son of Pakher, son of Osorkon, son of Takelot, son of Osorkon, son of Sheshonk, son of Shedsunefertem, the HPM named in table 2 (item 1.9) above. It follows that if we can date this Ashakhet, and if we can determine the approximate number of years in each generation of these high priests, we can work backward to establish a rough dating for Ramses II.

The dating of the Louvre stele is simplified somewhat by the fact that Mariette, its discoverer, assigned it, probably on stylistic grounds, to the time of Bocchoris, approximately 610-600 B.C. in the revised chronology. Traditional historians discount this creditation, arguing that the time of Bocchoris is much too late to complement other data of the traditional history. We argue that Mariette has dated it about twenty to thirty years too early. It was found near the entrance to the lesser vault of the Serapeum, and any explanation of its date must take account of the fact that it was not found within the confines of any vault occupied by a deceased Apis. It appears to have been deposited in haste. The location of the bull which died during the reign of Bocchoris is well known (tomb S), and it contained several stelae. Ashakhet's was not among them. Ashakhet cannot date precisely to that time. Another bull, two generations removed, supposedly died early in the reign of Taharka, only shortly before the invasion of Nebuchadnezzar in the revised chronology (perhaps in the initial stages of that conflict). Considering the location of Ashakhet's stela it is tempting to date his priesthood in this time frame, around 565 B.C. If so then we should date the birth of Ashakhet around the year 600 B.C. This is, of course, a rough approximation, but sufficiently accurate to prove the point we are about to make.

There remains the task of determining the length of a generation in the structured lives of the priests of the god Ptah. Initially we are inclined to adopt, for various reasons, a figure of around 15 or 16 years. These priests were very likely among the first born of their respective families, and

they were likely set aside to perform their lifelong tasks at a very early age (witness the fact, which we observed in *Piankhi the Chameleon,* that the youthful Harnakht, son of Osorkon II, was already, at the age of 7 or 8 years, designated as a high priest of Amun). We can assume that they were provided with a wife in their early teens, and likely produced their first offspring by their mid-teens. This is not all speculation. The genealogies themselves provide a clue.

We can see already in the table 2 genealogy that four generations of priests from this same family served during the 66 plus years of the reign of Ramses II. Were we to extend the genealogy upward in time two more generations we would see that these two forefathers served under Seti I, Ramses's father. Seti is only credited with 10 years of independent rule by the traditional history. Taking the two reigns together we see six consecutive generations of priests serving kings whose combined reigns spanned at most 77 years. Clearly our estimated figure is not excessive. In the table below we list the entirety of the Louvre genealogy as extended by the Berlin genealogy back through the reign of Ramses II. Allowing 16 years per generation, and assuming that Ashakhet was born around 600 B.C., we arrive at approximate dates for the birth of each priest. The fact that our assumptions are reasonably accurate will be borne out as we examine the results. Henceforth we will consistently refer to this combined genealogy as the *Berlin Genealogy* or the *Genealogy of Ashakhet*. On the assumption that it summarizes the meticulously kept records of the priests of the Memphite cult of Ptah, it will be utilized extensively in this book and the next. There is no justifiable reason to assume it is errant in any respect. Thus it is that both books utilize it extensively and consequently that both bear the subtitle "*The Genealogy of Ashakhet*".

Table 3: Genealogy of Ashakhet (C) from
Serapeum stele Louvre Cat. #52,
extended via the Berlin stele

Priest (Berlin and/or Louvre)	Approximate Year of Birth	Name of King Served
Ashakhet C	600 B.C.	---
Pakher	616 B.C.	---
Osorkon	632 B.C.	---
Takeloth	648 B.C.	---
Osorkon	664 B.C.	---
Sheshonk	680 B.C.	---
Shedsunefertem	696 B.C.	[Hedjkheperre Sheshonk]
Ankhefensekhmet A	712 B.C.	---
Ashakhet B	728 B.C.	---
Pipi B	744 B.C.	-- Psebkhannu (Psusennes I)-
Harsierse J	760 B.C.	Psebkhannu (Psusennes I)
Pipi A	776 B.C.	Aakheperre Setepnamun
Ashakhet A	792 B.C.	Amenemnisu (?) [=Amenmesse Hekawaset?]
Ptahemakhet	808 B.C.	---
Neferrenpet	824 B.C.	Usermaatre Setepenre (Ramses II)
Ptahmakhet	840 B.C.	Usermaatre Setepenre (Ramses II)
j[---]nashanet?	856 B.C	Usermaatre Setepenre (Ramses II)
Ptahhotep	872 B.C.	Usermaatre Setepenre (Ramses II)

There remains for us to comment on the general reliability of the data supplied by this table. At least four generations of this genealogy are attested elsewhere on assorted monuments. Again Kitchen speaks to this issue, noting that ...

> some first-hand confirmation of this genealogy and its dates of dignitaries is available. Happily, the high priest Shedsunefertem is securely attested in office under Shoshenq I, in the early 22nd Dynasty. He named his own son Shoshenq (who succeeded him in office), after his royal patron and relative-by-marriage; his own father is likewise confirmed as the high priest Ankhefensekhmet (A). Two generations before Ankhefensekhmet and Shedsunefertem, the high priest Pipi B is attested under Siamun on a temple building at Memphis, where the well-sculptured gateways included a lintel of the high priest 'Neterkheperre Meryptah who is called Piupiu'. He attends on Siamon in whose honor he adopted this loyalist name which is based on the king's prenomen. TIP 152

We take issue with only two of Kitchen's statements. The Hedjkheperre Sheshonk of which he speaks (calling him Shoshenk I) is not the first king of the 22nd dynasty, as we have previously argued.[4] We have assigned to him the years 681-660 B.C., in perfect agreement with our hypothesis that Shedsunefertem was born around the year 696 B.C. The inscription about which Kitchen speaks may well come from the last years of Sheshonk's reign, which would mean that Shedsunefertem was in his early thirties at the time, consistent with the fact that his father Ankhefensekhmet was apparently still alive and active.

We also question the identifications in Kitchens second remark. We can as easily consider the high priest in question to be Pipi A rather than Pipi B, and as we argued in our previous book we believe that the king Neterkheperre Siamon should be identified as Smendes I, the founder of the 21st dynasty, not Psinaches as Kitchen is arguing. We have dated Smendes to the years 760-734 B.C. and the birth of Pipi A to the year 776 B.C. The fit is perfect.

Traditional historians generally (though not unanimously) identify the two references to Psebkhannu as allusions to Aakheperre Psebkhannu, the first Psusennes in that historical scheme. We agree that we are here

[4]See Appendices A & B in *Piankhi the Chameleon*.

dealing with Psusennes I, though we believe that this is the Tyetkheperre Psebkhannu of the monuments. We have assigned to him the years 734-688 B.C., a time frame which agrees perfectly with the assumed birth dates of the two pontiffs who served him. Harsiese J (b. 760) and Pipi B (b. 744) would each have been around 40 years of age at the end of the first and third quarters of Psusennes' reign.

As for the Aakheperre Setepnamun who was served by Pipi A we again find substantial agreement with the chronology of table 3, though here some explanation is required. The traditional history typically identifies the king mentioned here as Aakheperre Psebkhannu, as in the two adjacent generations which provide the nomen, instead of the prenomen of this king. That is, of course, impossibility in the revised history, wherein the rationale for much of the argument depended on placing Aakheperre Psebkhannu at the end of the 21st dynasty, not near its beginning. Even many Egyptologists question why the author of the inscription would refer to Psusennes by his prenomen once and his nomen twice, and thus they assign the reference in question to Psusennes I and the two following instances of the name Psebkhannu to Psusennes II (thereby effectively discounting the reliability of this genealogy as an historical source). Fortunately we have an explanation ready at hand.

There are two *known* Egyptians kings who had the identical prenomen Aakheperre Setepnamun - Psusennes I (our Psusennes II) and the 22nd dynasty king Osorkon IV to whom we assigned the dates 618-607 B.C. in *Piankhi the Chameleon*. We might add to this list Osorkon's predecessor Sheshonk V, whose prenomen was similar (Aakheperre Setepenre). Kitchen also documents the existence of another Aakheperre Setepenre, whom he tentatively identifies as Menetho's 21st dynasty king Osochor. (TIP 4, 388 #56) It can be argued that the names Setepenre and Setepnamun are merely variants of one another.[5] Apparently the prenomen Aakheperre followed by one or the other variant Setep-en-

[5]In his discussion of the king named variously Usimare Setepnamun and Usimare Setepenre, Kitchen comments: "Differing solely in the use of either Amun or Re in the complement Setep-en-x, 'Chosen of (the god) x', Nos. IIIa and b have been well discussed long since, and their identity affirmed, by Gauthier, an identity which is now generally accepted." TIP 66 Amun and Re were the same god; the only change is in which of the variant names is used.

Amun/Re was an extremely popular name in the late 21st/22nd dynasty time frame, where (as we argued in the earlier book) names were frequently borrowed from illustrious predecessors. Keeping this in mind, and also bearing in mind our discussion in Piankhi concerning the three enigmatic kings who founded the 22nd dynasty, we have the likely solution to our problem. We have dated the 22nd dynasty kings Sheshonk I, Osorkon I, and Takeloth I, to the years 760-740 B.C., precisely in the two decades which preceeded the reign of Psebkhannu I. We have also argued that we do not know the prenomen of any of these kings. It should surprise no one to discover that one of them bore the prenomen Aakheperre Setepnamun, which name was borrowed in the next century by at least two, and possibly as many as four kings. Assuming that the name Aakheperre Setepnamun refers to either Sheshonk I or Osorkon I, it is not surprising that this king was served by Pipi A who was born in 776 B.C. Nor should it be surprising that this same Pipi served Siamon of the 21st dynasty. The two competing dynasties had only recently come into existence, and the overlapped one another. These were difficult times in which a symbiotic relationship likely existed between the delta kinglets of the 21st and 22nd dynasties.

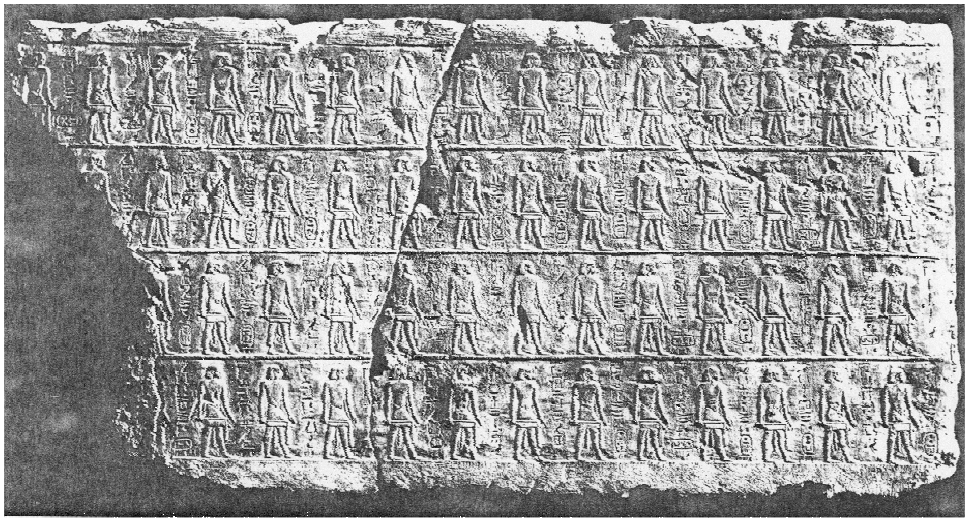
Only one piece of information in the entire listing in table 3 is problematic for the revised history (as it is also for the traditional history). On the Berlin stele (see figure 2 below), at the left end of the uppermost register, we find the 15th generation name of the king served by Ashakhet A (thus numbered 1.15 in Kitchen's table for 1st row, 15th element reading right to left) transliterated Amenemnisu by Kitchen (following Grdseloff and Kees)[6] but Amenophthis by Borchardt, who first published the Berlin document. [7] Borchardt transliterated the cartouche name, which unfortunately lies near the broken left end of the inscription, as 'Imn-m-ip-t-rs-t, a considerably different reading than that provided later by Grdseloff and Kees. All three scholars suffer from the same handicap. They are attempting to read a 21st dynasty name into the damaged section of hieroglyphs, one believing that the name must refer to Manetho's Amenophthis (identified as Amenemepet by the traditional history), the other Manetho's Nepherkare (Amenemnisu). But the orthography actually

[6]Cf. Kitchen TIP 152 n.6 for bibiliography.
[7]Ludwig Borchardt, *Quellen und Forschungen zur Zeitbestimmung der Agyptischen Geschichte* (1935) 96-112.

resembles neither name as found elsewhere. We believe that the reading Amenmesse Heka-waset may be the correct reading but confess that we are influenced in our judgment by the revised chronology and the desire to read here a name from the period of civil unrest which followed the death of Ramses II. It may well be that the king named here is otherwise unknown to historians. It could be one of Ramses many son's, some of whom no doubt contested for power but left no other record of their existence. We note that Ramses did have a son named Amenemopet with orthography close to what is visible in the inscription. Perhaps Borchardt was correct in the reading but wrong in the assignment of the name. We leave the matter there.

One last argument for the accuracy of our historical revision is clearly provided by the Berlin genealogy. The second row of names begins on the extreme right (2.1) with a high priest who served an unknown king (perhaps Merenptah) followed by four others (2.2-2.5) who served Ramses II. Since we have dated Ramses II to the years 840-774 B.C. there is perfect chronological agreement with the revised history. The four priests who functioned under Ramses were born between the years 872-824 B.C.. Conceivably all four could have been in their mid to late 30's when they served at various times under Ramses II.

Figure 2: The Berlin Stele Genealogy



This discussion of Ramses II brings to the fore the obvious question for which the traditional history has no *reasonable* answer. Why is the 21st dynasty (21st/22nd dynasties according to our interpretation) preceeded immediately by the 19th dynasty in the *Berlin Genealogy*? What happened to the 20th dynasty?

The problem has been debated since Borchardt first published the inscription. Kitchen acknowledges the fact that "for roughly 150 years or more from the death of Ramesses II to the early 21st dynasty, the Berlin document has only one generation (Ptah-em-akhet B) between Ramesses II and Amenemnisu" (TIP 153) He goes on to suggest that "it is theoretically possible to argue a simple omission of six or seven names (a haplography?), or even that further names in the top row are lost on some slab that was once contiguous with it", and then he goes on to point out other flaws in the Berlin sequence that cannot be so easily dismissed. But we should not pass by Kitchen's explanation so quickly. Neither proposal is even remotely possible considering the structure of the genealogy and the careful planning that has gone into its construction.[8] And we challenge Kitchen to cite chapter and verse of any other inscription where such a lengthy haplography has occurred. Perhaps a single hieroglyph; occasionally a word or phrase; but a whole line of text and figures on a block which in all probability was carefully drawn out on the polished surface before the artisan inscribed it - impossible! And why, we ask, did the distinguished priestly family, whose genealogy is here reproduced, not object to the supposed omission?

This stele at minimum shows the bankrupt state of the traditional chronology and thus of the history enmeshed within it. And it establishes, as does no other single document, the essential reliability of the relative chronology of the revised history. And the mere fact that we *could* construct a genealogy *with dates included*, as in Table 3, which explains

[8]The notion that a slab precisely the depth of only the top row in the inscription was originally present and that it extended precariously to the left but has subsequently broken off, is, frankly speaking, absurd. The left side of the block is clearly preserved to its original extent on the first row. To my knowledge there exists no other monument among the tens of thousands of extant monuments in Egypt that is constructed with the odd dimensions suggested by this remark. To his credit Kitchen later states his preference for the alternative suggestion, i.e. the haplography.

every aspect of the Berlin and Louvre genealogies, goes a long way to proving the essential reliability of our absolute chronology.

In passing we should answer one anticipated objection to the discussion related to the High Priests of Ptah in Memphis (HPM). The critic will surely point out that there are other high priests of Ptah which are not included in the Louvre and Berlin documents, names such as Sheshonk, Merenptah, and Pediese to name a few. In the traditional history these are appended to the end of the Louvre genealogy. The claim is made by the traditional history that the Louvre and Berlin lists take us into the 22nd dynasty at the time of Shedsunefertem, this on the assumption that Hedjkheperre Sheshonk, the contemporary of Shedsunefertem, is Sheshonk I, the founder of the Dynasty. It is then claimed that the remaining names on the list take us further into the 22nd dynasty (roughly to the time of Osorkon II) and that the additional high priests of which we speak (one, the son of Osorkon II, one from the time of Takeloth II, one a contemporary of Sheshonk III, etc) continue through the balance of the dynasty.[9] But according to the revised history the last priest of the Louvre stele lived decades beyond the close of the 22nd dynasty. There is no room for additional high priests. Then where do we place these superfluous names? The answer is simple. We place them alongside the names from the Louvre and Berlin genealogies. Throughout much of the 22nd dynasty period there must have existed at least two high priests of Ptah, one in Memphis to be sure, but also at least one elsewhere, possibly though not certainly in Bubastis, associated more intimately with the 22nd dynasty kings. After all, this was the age of competing dynasties. None of the titles of any of these high priests contains a specific mention of the city of Memphis as if to imply that they must be associated with the Ptah cult centered there. Enough said.

[9]We can see why Kitchen is insistent on a lengthy reign for Osorkon II. He has to fit the six generations which follow Shedsunefertem in the Louvre genealogy between the reigns of Sheshonk I and the end of the reign of Osorkon II. The reader should keep that fact in mind should Egyptologists question the 15 years we have assigned to each generation of Memphite priests, claiming it to be too low. Kitchen's scenario assumes a figure much less than ours.

Chapter Two
A Backward Glance: The Late 18th Dynasty

The Berlin Genealogy: A Second Look

In chapter one we suggested and briefly defended approximate dates for the 67 year long reign of Ramses II (840-774 B.C.). In support of these dates we referred to the genealogy of Ashakhet, a high priest of Ptah who was born, according to our estimate, around the year 600 B.C. Working backward in time from that date using the Louvre and Berlin stelae, and allowing 16 years per generation, we reasoned that the four ancestors of Ashakhet who served Ramses II must have been born around the years 872, 856, 840, and 824 B.C. respectively. Assuming that their high priesthoods were reached by the age of 35, we concluded that they served Ramses II beginning around the years 837, 821, 805 and 789 B.C., and that the *Berlin Genealogy* was therefore supportive of our assumed dates for this important 19th dynasty king.

We recognize the tenuous nature of the assumptions which guided these calculations, but the fact that they produced reasonable correspondences to kings belonging to the 21st and 22nd dynasties included earlier in the genealogy lent credence to our methodology. And it suggested to us, though we did not immediately act on the suggestion, that the *Genealogy of Ashakhet*, extended further back in time by using additional data provided by the Berlin stela, might be useful in extending our historical revision backward to the beginning of the 19th dynasty, and perhaps even further, touching base with the terminal kings of the 18th dynasty, Amenhotep III, Akhenaton and Haremheb. We now act on that earlier impulse. Though this backward glance temporarily interrupts our focus on the eighth century, it is expedient that it be undertaken sooner rather than later, if only to answer questions which the discussion in chapter one might have raised in the minds of informed readers.

Several times in the previous two books of our series we have mentioned the revisionist work of Immanuel Velikovsky, who in 1952 published an exhaustive reappraisal of 18th dynasty chronology in his pivotal *Ages in Chaos*. In that book Velikovsky argued that the queen of Sheba, a

contemporary of Solomon, was none other than queen Hatshetsup, the predecessor of the 18th dynasty king Thutmose III. It followed from that identification that Thutmose III must have been a contemporary of Solomon's son and successor Rehoboam and that *he* must therefore be identified as the biblical pharaoh Shishak who sacked Jerusalem early in Rehoboam's tenure in office. It also followed that Amenhotep II, Thutmose's grandson, must be the biblical Zerah, the Ethiopian general who lost a battle to king Asa of Judah around the year 900 B.C. Velikovsky further reasoned, allowing a 55 year reign for Thutmose III, that Amenhotep III, Akhenaton and Tutankhamon must have ruled Egypt in the mid-ninth century, and that the famed Amarna letters, which date from late in the reign of Amenhotep III and run through the reigns Akhenaton and Tutankhamon, must belong in the time frame 870-840 B.C. Clearly our chronology, which fixes the beginning of the reign of Ramses II around the year 840 B.C., stands in conflict with Velikovsky's thesis. Velikovsky's schema leaves us no room for the 30 year reign of Haremheb and the several decades which belong to Ramses I and Seti, the immediate ancestors of Ramses II. Some, though not all of Velikovsky's error results from his assigning to Thutmose III an excessively long reign of 54 years, following the traditional history. As explained in the second book of our series, this king did not author the Karnak annals which bear his name, those which, in conjunction with the memoirs of his army general Amenemheb, assign to him this expansive reign. Thutmose's kingship was much shorter, certainly less than twenty years, a fact soon to be confirmed by the Berlin genealogy where his name occurs only once. This suggested shortening of Thutmose's reign length, in and of itself, does not account for the earlier dating of the Amarna letters which we are about to suggest, but it is without doubt a major contributor.

Returning to the Berlin genealogy, we reproduce below the data from the second row (positions 2.1 through 2.15) and the beginning of the third row (positions 3.1 -3.4) of ancestors, reading, as always, from right to left on the monument. We supply additionally the approximate date of birth and date in office of each of the priests, continuing to use the 16 year and 35 year figures which yielded reasonable results earlier in the genealogy. In evaluating the table we must be mindful of the provisional status of the resulting chronology, the chances of error increasing the further we are

removed from the time of Ashakhet. But as it stands, the suggested chronology is illuminating, and is probably correct to within a few years.

Table 4: The Berlin Genealogy Chronology Extended

Position Number	High Priest/Prophet Named	Name of King Served	Approx. Date of Birth	Approx. Date of the H.P./Prophet
2.1	Ptahemakhet	not named	808 B.C.	773 B.C.
2.2	Neferrenpet	Ramses II	824 B.C.	789 B.C.
2.3	Ptahemakhet	Ramses II	840 B.C.	805 B.C.
2.4	[--]masjemet?	Ramses II	856 B.C.	821 B.C.
2.5	Ptahhotep	Ramses II	872 B.C.	837 B.C.
2.6	Neterwihotep	Menmaatre (Seti I)	888 B.C.	853 B.C.
2.7	Sekeremsaef	Menmaatre (Seti I)	904 B.C.	869 B.C.
2.8	Ty	Tjeserkheperre (Haremheb)	920 B.C.	885 B.C.
2.9	Sekeremsaef	not named	936 B.C.	901 B.C.
2.10	Aypew	Ietnetjeray (?) (Ay)	952 B.C.	917 B.C.
2.11	Wirmer	Nibmaatre (Amenhotep III)	968 B.C.	933 B.C.
2.12	Penpanebes	Nibmaatre (Amenhotep III)	984 B.C.	949 B.C.
2.13	Nekhememptah	not named	1000 B.C.	965 B.C.
2.14	Nebre	Menkheperre (Thutmose III)	1016 B.C.	981 B.C.
2.15	Damaged section		1032 B.C.	997 B.C.
3.1	Ty	not named	1048 B.C.	1013 B.C.
3.2	Pa'emrud	Djeserkare (Amenhotep I)	1064 B.C.	1029 B.C.
3.3	Ty	not named	1080 B.C.	1045 B.C.
3.4	Menet	Nebpetire (Ahmose I)	1096 B.C.	1061 B.C.

It goes without saying that the date assigned to each king in this table represents nothing more nor less than an approximate date at which that particular king was likely reigning in Egypt. It need not imply, for example, that the king in question ruled for the entire sixteen years assigned to that generation, nor that the absolute date associated with the priest's service to a particular king is central in the span of years during which that king actually reigned. But with that in mind the table remains instructive in confirming several assumptions we have made in our earlier discussions vis-a-vis the 18th dynasty kings.

In the first instance we cannot help but observe an obvious discrepancy between the length of the 18th dynasty as indicated by the Berlin genealogy, and the length of that same dynasty as represented in the textbooks sanctioned by the traditional history. Since position 3.5 in the Berlin document is occupied by a priest who served under Apophis, the terminal king of the 17th (Hyksos) dynasty, it is clear that the 18th dynasty patriarch Ahmose I began his rule at the earliest some time around the year 1073 B.C.. At the other end of that dynasties span of years, the rule of Haremheb, which brought the dynasty to a close, must be dated at latest around 873 B.C.. The resulting 200 years assigned to the 18th dynasty contrasts markedly with the 267 years assigned these same kings by the traditional history. Contemporary historians typically place the dynasty roughly in the time frame 1575-1308 B.C.

The difference of sixty-seven years cannot be accounted for by simply lengthening the time of each generation in the Berlin genealogy by six or seven years. If we were to make such a change it should apply consistently to all generations. But we have already observed how, at the lower end of the time frame under consideration, any increase would produce serious chronological problems for the four generations of priests who served Ramses II and the two preceding generations who served Seti I, whose independent reign, as is, must necessarily be increased from the ten years assigned it by the traditional history. And at the other end of the spectrum the length of a generation of priests must be correspondingly small. Here we observe that three generations of priests served the combined reigns of Ahmose I and his successor Amenhotep I (positions 3.2-3.4). Since Ahmose ruled around 22 years, and Amenhotep at most an additional 21 years, it follows that the 16 years per generation figure is, if

anything, excessively large. Any significant lengthening of the generation number cannot be justified.

Thus at both ends of the genealogy depicted in table 4, our figure of 16 years per generation seems about right. There is therefore no justifiable reason for adopting an alternative number at any point between the extremes we have discussed.

The only reasonable conclusion to be drawn from table 4 is that the length of the 18th dynasty as represented in the traditional history is excessive and needs to be reduced by around 65 years. And the table itself suggests the means whereby this reduction should be made. It is clear from even a casual glance at the Berlin chronology that a drastic shortening of the reign of Thutmose III is demanded, and a modest reduction in the combined reign lengths of Thutmose's three predecessors is strongly suggested. The latter can be accomplished in part by allowing only a year or two for the reign of Thutmose I.

The reader will recall from our discussion in *Piankhi the Chameleon* that the mummified remains of Thutmose I and Thutmose III do not support the excessively high number of regnal years assigned them by the monuments which bear their names. The Berlin chronology adds support to our earlier argument that the monuments bearing the names of these two kings, those which support the inflated regnal numbers assigned them in the traditional history, belong instead to 7th century namesakes. In table 4 the two generations which precede Menkheperre Thutmose must accommodate the reigns of three kings - Thutmose 1, Thutmose II and Hatshepsut. Gardiner credits Thutmose II with 18 years and Hatshetsup with 20. As is, these two reigns more than fill the time allotted to them on our assumption of 16 years per generation. If there existed no co-regency between Thutmose II and Hatshetsup then their reigns must necessarily intrude slightly into the time slots assigned to Amenhotep II and Thutmose III, leaving little if any room for Thutmose I. The reign of Thutmose I must indeed have been brief, as his mummy suggests. We should credit him with perhaps a year of two of independent rule at most. Monuments which suggest otherwise must belong to someone other than the 18th dynasty king. Either that, or a lengthy coregency with his father and/or with his son must be assumed.

We note also from our table that the generation following the mention of Menkheperre Thutmose must encompass the reigns of Amenhotep II and Thutmose IV, assigned 23 and 8 years respectively by the traditional history. Once again they must intrude on the time slots of Menkheperre who precedes them and Amenhotep III who follows. If our reasoning is correct, the reign of Menkheperre cannot have been even a generation (16 years) in length. If so, there can be no denying the reasonableness of our earlier conclusions regarding the Annals which bear his name. Those Annals must belong to someone else. Under no circumstances can a 54 year long reign for this king be squared with the data on the Berlin stele, regardless of how many years we assign to a generation of priests!

The Amarna Age

The Berlin chronology presented in table 4 tells us that Amenhotep III was ruling Egypt minimally from 949-933 B.C. plus some undetermined years at either end of that span. Since the monuments clearly assign a minimum of 37 years to this king we are compelled to add an additional twenty-one years. And since, according to our table, Ay was on the throne in the year 917 B.C., we are compelled also to assign most of those years to the upper end of the span of years, leaving as much space as possible at the lower end (between Amenhotep III and Ay) in which to insert the reigns of Amenhotep IV (17 years), Smenkhkare (3 years) and Tutankhamun (9 years). Tentatively we assign to Amenhotep III the years 966-928 B.C. Even at that the genealogy all but demands that we assume a lengthy period of coregency between Amenhotop III and his son Amenhotep IV (Akhenaten), a possibility much discussed and disputed in the literature. For the time being we assign to Akhenaten the years 940-923 B.C. The genealogy further suggests that the years of Smenkhkare must have overlapped the reign of Tutankhamon, another subject much debated. We cannot be far wrong in assigning to Tutankhamon a brief 5 year reign in the approximate time frame 923-918 B.C., his remaining 4 years overlapping the reign of Ay who usurped the throne, and to whom we assign the years 918-914 B.C. Only by assuming these co-regencies

can we possible accommodate the reigns of the three kings in the time available between Amenhotep III and Ay.[10]

Table 5: Tentative Dates for the Late 18th Dynasty Kings
(based on the Berlin Stele genealogy)

Name	Regnal Years
Amenhotep III	966-928
Amenhotep IV (Akhenaton)	940-923
Smenkhkare	923-920
Tutankhamun	923-914
Ay	918-914
Horemheb	

Before proceeding we should acknowledge one last time that the suggested absolute chronology for the 18th dynasty kings which results from the Berlin genealogy can only be considered an approximation. The "iffy" nature of the assumptions which undergirded our reasoning and the vexed questions regarding co-regencies, suggest the need for caution regarding the conclusions that we now draw from an examination of that chronology. In the final analysis it will be the ends that justify our means. The Berlin genealogy as interpreted above clearly places the Amarna age, which ranges from the terminal years of Amenhotep III through the reigns of Akhenaten and Tutankhamon, near the end of the 10th century B.C., somewhere in the time frame 930-910 B.C., and perhaps a few years beyond. It is incumbent on us to show the reasonableness of those dates, thus vindicating our methodology.

The Jewish Background of the Amarna Letters

If Amenhotep III ruled Egypt in the time frame 966-928 B.C., then it must be the case that he ruled as a contemporary of the Israelite king Solomon (970-930 B.C.) and briefly into the reigns of Solomon's son and

[10]It is important to note that these coregencies must be assumed even if the generation figure were increased by five or six years.

successor Rehoboam (930-913 B.C.), and Rehoboam's northern rival and antagonist, Jeroboam I (930-908 B.C.). It must also be true that Amenhotep IV (Akhenaten) (940-923 B.C.) began joint rule with his father late in the reign of Solomon, succeeded his father early in the reign of Rehoboam, and ruled as a contemporary of Rehoboam (and Jeroboam) for much of his life. We cannot be more specific than this. There are simply too many variables, including the Jewish chronology which is followed here.

Fortunately this "Berlin chronology" places the Amarna age squarely within the kingdom age of Israel, more specifically, at the beginning of the divided kingdom period (930-722 B.C.). It should be possible therefore to confirm its accuracy by comparison with the political history of Israel as revealed in the Hebrew Bible.

According to Jewish historians, Jeroboam dispossessed Rehoboam as the leader of the northern Palestinian tribal groups immediately following the death of Solomon, an action which began the prolonged period of the divided monarchy. Earlier Jeroboam had had a falling out with Solomon and had fled to Egypt to seek the protection of an Egyptian pharaoh named Shishak. While there, according to one late source, he married a sister of Shishak's wife.[11] When Solomon died Jeroboam returned to Israel and almost immediately led a coalition of northern Israelite tribes in rebellion against Rehoboam, establishing a rival kingdom. It follows that Egypt, whom we assume supported Jeroboam's rebellion, thereby established dominance in the region, receiving tribute from Jeroboam and the other tribal leaders, while Rehoboam for a time maintained the independence of the southern tribes. This temporary independence was brought to an end four to five years later (925 B.C.) when Shishak, for reasons unknown, attacked Rehoboam, overthrew Jerusalem, and pillaged the treasury of the Jerusalem temple (1 Kings 14:25,26). While Rehoboam remained as titulary head of the southern tribal region until his death twelve years later, we assume that Egypt maintained a military presence in the Jerusalem area, and perhaps as well imposed

[11]This late midrash added to the biblical text is found in the Septuagint version of 1 Kings 12:24. The text of the Sepuagint begins by following the Massoretic Hebrew text, then diverges into a lengthy narrative describing the exploits of Jeroboam, then returns to follow the Hebrew Bible in 1 Kings 12:25.

secondary political control. Thus by the year 925 B.C. we find in Palestine a quite distinctive political situation. The overriding authority is clearly that of Egypt, with whom Jereboam is aligned, possibly by marriage and certainly by sentiment, and under whom Rehoboam served as a reluctant vassal. The land was partitioned into two distinct tribal regions with an east-west boundary line roughly located along the Jezreel valley. For the next fifteen years Jeroboam and Rehoboam clashed ideologically, and at times militarily, as each attempted to unseat the other. The Jewish historians are silent regarding the role of Egypt. Its presence is assumed, but not defined. Two identifications follow directly from this chronology.

In the first place we are compelled to identify pharaoh Shishak with either Amenhotep III or Akhenaton, who ruled jointly during the years immediately preceding the death of Solomon and alone qualify as candidates for the pharaoh who befriended Jeroboam. If we had complete confidence in the regnal dates assigned earlier we should rule out Amenhotep III, whose death in 927 B.C. preceded Shishak's attack on Jerusalem by several years. But those dates remain open to question, as do the precise absolute dates for Rehoboam's reign. We should therefore maintain the viability of both possibilities.

Secondly, the biblical figure Zerah the Cushite who clashed with Rehoboam's grandson Asa around the year 900 B.C. (2 Chron. 14:9-13) must be Horemheb, who succeeded Ay around 914 B.C. and ruled Egypt for 27 years.

We will spend no time defending these identifications. As did Velikovsky fifty years ago, we base our assumptions solely on chronological synchronisms supported on other grounds, leaving defense of this aspect of our thesis to others, or to ourselves at a later date. Sufficient here to note that in the Amarna correspondence from Palestine the pharaoh is never addressed by his cartouche names, rather by some honorific epithet that alludes to his splendour, usually Shimshi, "my Sun". An original alteration of Shimshi to Shimshak "your Sun" (hence over time to Shishaq) may have been a deliberate circumlocution on the part of scribes who passed on the Jeroboam story through the ages until its final post-exilic editing four hundred years after the fact. The fact that the "mem" elided

over time and that terminal "qoph" replaced the "kaph" cannot be considered a strong objection. In the transmission of names it was the sound that was of paramount concern. As for Zerah, we can only surmise a guess that the initial part of the throne name **Hera**-em-heb was the possible source of the biblical phoneme, though we have no explanation for the alteration of "h" to "z". The fact that Zerah is not identified as a pharaoh is consistent with the non-regal status of Horemheb early in his career. He began as a commander of the army under Tutankhamon and Ay. Precisely when he assumed pharaonic status is not known for certain, and ambiguity regarding his rank may explain the lack of title accorded him by the Jewish historians.

There remains for us to summarize the Amarna letters from Palestine and compare the situation therein revealed with the emerging divided monarchy described in the Jewish historical books.

The Amarna Correspondence

It is most fortunate for our argument that the "Berlin chronology" places the Amarna correspondence in the time frame 930-910 B.C. In that correspondence we find multiple references to the city of Jerusalem, the name assigned to the southern capital by king David, the father of Solomon. The beginning of the reign of David, approximately 1010 B.C. in the Jewish chronology we are following here, thus acts as a *terminus a quo* for the Amarna letters. On the other hand Shechem, the most prominent city of the northern kingdom in the Amarna letters, yielded its dominance in time to Samaria, a city founded by king Omri, a near descendent of Jeroboam, around the year 885 B.C. The fact that Shechem remains dominant in the letters, while Samaria is never mentioned in spite of a prolific correspondence from cities in the same geographical area, implies that the reign of Omri lies yet in the future, providing a *terminus ad quem* for the Amarna correspondence.

But not all dates between 1010 B.C. and 885 B.C. would be appropriate contexts for the Amarna letters. That correspondence, as we will soon see, clearly represents Egypt as the dominant power in the Levant, sovereign over the entire eastern coast of the Mediterranean, from the Philistine

cities on the southern coastal plain to Simyra and Biblos on the northern coast, and inland as far as Kadesh on the Orontes. Though its sovereignty is being threatened in the north by the emerging power of the Hittite and Mitannian kingdoms, and by internal feuds wherein at least one powerful family threatens to usurp the authority of the semi-autonomous city nomarchs, Egypt continues to rule the Levant throughout the brief duration of the Amarna correspondence. This situation could clearly not arise during the reigns of kings David and Solomon, whose kingdoms controlled not only the Phoenician coastline, but the Syrian hinterland as far as the Euphrates.[12] The Amarna letters must postdate the reigns of both David and Solomon. They must date therefore between the years 930 B.C. and 885 B.C., the onset of the reign of Omri. And even these limits should be further narrowed. In the tenth year of Asa, who succeeded Jeroboam after the brief tenure of Abijam (Jeroboam's son), thus roughly 900 B.C., Egypt's sovereignty over the Levant was abruptly terminated, when Asa defeated and expelled the Egyptian army led by Zerah. The Amarna letters must date therefore in the thirty year interval from 930-900 B.C. No earlier or later date would be acceptable. Is it mere chance that the Berlin chronology positions the Amarna age precisely in this narrow temporal window of opportunity? Let the reader decide.

The question remains how precisely the data from the Amarna letters fits the known circumstances of the initial decades of the divided monarchy?

The Rebel Labaya

When Velikovsky discussed the Amarna correspondence in his *Ages In Chaos,* and attempted to compare the data contained therein with the historical situation which prevailed during the time of Ahab the son of Omri in the middle of the 9th century, he made two serious mistakes. In the first place he read the letters emanating from Sumura, a coastal town near Gubla (Biblos) on the northern shore of the Mediterranean, as if the

[12]As for Solomon we are told that "he ruled over all the kingdoms west of the River (i.e. the Euphrates), from Tiphsah to Gaza, and had peace on all sides. During Solomon's lifetime Judah and Israel, from Dan to Beersheba, lived in safety, each man under his own vine and fig tree." 1 Kings 4:24,25.

name referred to Samaria, the inland capital of the northern kingdom of Israel founded by Omri. Sumura in the letters is clearly the Phoenician port city of Simyra, located hundreds of miles to the north. But a more serious error, this time of omission rather than commission, was his failure to discuss the activity of the most prominent character in the southern correspondence - the rebel Labaya. One is hard pressed to find a reference to this individual in the whole of Velikovsky's work[13], rather surprising, since Labaya figures directly or indirectly in almost every letter. We need to set forth the details relating to this intriguing character, comparing them at every turn with the life and times of Jeroboam, son of Nabat. We state our intention at the outset, namely, to demonstrate that Labaya and Jeroboam are one and the same person. If not, we are seriously mistaken in our temporal placement of the Amarna correspondence.

Political Background

We have already noted the involvement of Egypt in the split that emerged between the northern and southern Israelite tribes at the time of Solomon's death. Though not discussed in the Jewish historical literature, where the activities of Jeroboam and Rehoboam are summarized in a few succinct paragraphs, the dominance of Egypt in the region is nevertheless understood, particularly in the aftermath of Shishak's conquest of Jerusalem. The Amarna letters provide us a glimpse into this political background as it existed sometime late in the "reign" of Jeroboam. In order to understand much of the discussion that follows the reader needs to be informed concerning the broad details of this backdrop. Sufficient here to quote the words of the late William Albright, one of the world's most renowned experts on the Amarna documents, in his *Cambridge Ancient History* article on the subject:

> As far as practicable *the Egyptians had left the local princely houses in control of their own territories, but under the close supervision of Egyptian agents* whom

[13]Discussion of Labaya's activity is restricted to a single footnote of page 282 (note 3). Velikovsky views the activity of Labaya as a preliminary phase of the revolt of the town of Libnah which took place following the death of Jehosophat (2 Chron. 21:10) But Libnah is not mentioned in the Amarna literature and Labaya's actions and influence are far more expansive than suggested by Velikovsky.

> we may conveniently designate as 'commissioner' (Akkadian rabisu, Canaanite šokinu, Hebrew šoken) and "envoy" (Egyptian upitu [wpwty]). These agents were generally Egyptians, but they were not infrequently Canaanites of Semitic stock, as in the case of Iankhamu and Addayu. Sometimes native princes played an important role in Egyptian administration, as in the case of Iapa-Adda, who was probably prince of Tyre, or Piryawaza, prince of the Damascus region. The chief centers of Egyptian administration in Palestine were Gaza and Joppa on the coast; Gaza is mentioned several times as the residence of an Egyptian commissioner in one letter, and it appears already in that role in an earlier letter from Ta'anach. (CAH 2nd II part 2 p. 102) (italics added)

This scenario fits reasonably well the assumed political situation that prevailed in the days of Jeroboam and Rehoboam. From the point of view of the Hebrew Bible these two princes were fully in control of their own territory. Nothing is said about their subservience to a higher authority, a matter of little concern to the Jewish scribes, as elsewhere in the historical literature. The Amarna letters merely augment our understanding of the situation.

While Egypt discouraged rivalry between the local princes, it is clear in the Amarna letters that this rivalry did exist, at times erupting into armed conflict. And in the Palestine area the hostility was between the cities controlled by and loyal to an individual named Labaya, and cities loyal to Jerusalem. Clearly this is a subject of great interest to our thesis. The Hebrew Bible omits the details of the Jeroboam/Rehoboam rivalry, providing only a summary statement. A single paragraph describes the invasion of Shishak in Rehoboam's fifth year. The next paragraph describes Rehoboam's death.

> As for the other events of Rehoboam's reign, and all he did, are they not written in the book of the annals of the kings of Judah? *There was continual warfare between Rehoboam and Jeroboam.* And Rehoboam rested with his fathers and was buried with them in the City of David... And Abijah his son succeeded him as king. 1 Kings 14:29-31. (italics added)

For details of the warfare we must read the Amarna letters.

Geographical Background

Mid-way through his reign, perhaps sensing the possibility of internal revolt or attack from without, Solomon set about fortifying several key cities in the country, including Jerusalem.

> Here is the account of the forced labor King Solomon conscripted to build the LORD's temple, his own palace, the supporting terraces, the wall of Jerusalem, and *Hazor, Megiddo, and Gezer*. Pharaoh king of Egypt had attacked and captured Gezer. He had set it on fire. He killed its Canaanite inhabitants and then gave it as a wedding gift to his daughter, Solomon's wife. And Solomon rebuilt Gezer.) 1 Kings 9:15-17 [italics added]

From this and other texts we see that in addition to Jerusalem Solomon selected three cities in which to house the chariots and horses used by his standing army - Hazor, Megiddo, and Gezer. All three are mentioned in the Amarna letters.

Hazor is located in the far north of Palestine, near lake Huleh. It belonged traditionally to the tribe of Naphtali, and in the division of the kingdom at Solomon's death its allegiance would be with Jeroboam. Either this or it remained unaligned. The two letters which eminate from Hazor are uninformative. The head man of the city, Abdi-Tirši [14], used the opportunity to convey to the Egyptian king expressions of steadfast loyalty.

Megiddo, the coastal city at the western end of the Esdraelon valley, almost certainly remained part of Rehoboam's southern kingdom when Solomon died, though the Jewish historians are silent on the matter. Considering its strategic location it would have been a prime candidate for conquest by Jeroboam in the years of his warfare with Rehoboam. In

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[14]Letter 227 is written by an unnamed king (šarum) of Hazor; letter 228 by one Abdi-Tirši, who identifies himself merely as the "man of Hazura" (amêl Hazura). Whether the two individuals are one and the same, what precisely is the meaning of amêl Hazura (Albright would undoubtedly translate "chief of Hazor", a meaning he gives to amêl (Akkadian awilum = "man") elsewhere, and what is the status/rank of a šarum (some translators consider him to be the equivalent of a "mayor") are all important questions, but of little consequence in the discussion at hand. All large towns had a local chief; the more important towns acted as the political center of groups of smaller towns; much as in many countries today. The chief of Hazor proudly declares: "I protect Hazura together with its cities [fo]r the king, my lord." It is a moot point whether or not Hazor lay within the area of influence of Labaya at this point in time.

the Amarna letters its "mayor" was a man named Biridia[15], the author of at least six letters to the Egyptian king (nos. 242-246, 248a). Letters 244-246 complain incessantly about the threatening activity of Labaya. Letter 244 is typical:

> To the king, my lord and my sun (shimshi), say.
> Thus saith Biridija, the faithful servant of the king:
> At the two feet of the king, my lord and my sun (shimshi),
> seven times and seven times I fall down.
> Let the king my lord know that since the archers have returned
> Labaja (Labaya) has made hostility against me.
> And we are unable to mow the plants (kaziga),
> and we are unable to go out of the gate because of Labaja,
> since he learned (this)
> [A]nd (still) thou hast n[o]t [giv]en archers.
> [A]nd, behold, he has, [ver]ily, directed [his] endeavour to take Magidda
> But verily, let the king rescue his city, in order that Labaja may not conquer it.
> (244:1-29) [16]

Biridia goes on to request that Egypt send one hundred garrison troops to save his city from conquest by Labaya, a request ignored judging from later correspondence.

Letter 245 suggests that an Egyptian governor did subsequently intervene and order Labaya (lit "took him and sent him") to return to his home, presumably the city of Shechem[17], in spite of Biridia's plea that he be taken to Egypt. As we will observe in a moment, Labaya, though a constant threat to cities loyal to the pharaoh, was nevertheless dealt with leniently by the Egyptian king, and for reasons unknown accorded special treatment. If we are correct, and Labaya is indeed Jeroboam, a brother-in-law of the pharaoh, we understand why.

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[15]The ending of this name should likely be interpreted as a hypochoristic form of the name of Yahweh, the god of Israel. As such the name means "Yahweh thunders/storms". The same should be said for Lab'aia (Labaya), a name which means "Yahweh is a lion". This notion that the Akkadian ending "ia" should be so interpreted is absent from the commentaries for understandable reasons. The Amarna correspondence is typically dated to the 15th century B.C., at a time preceding the arrival of Israel in Palestine.The presence of this attenuated name of Israel's god is just one more indication that the traditional history has misdated the Amarna correspondence.

[16]All quotations from the Amarna letters, unless otherwise stated, are taken from the two volume edition of *The Tell-El-Amarna Tablets* edited by Samuel A.B. Mercer (1939).

[17]The editors supply the interpretive note "The home of Labaia was probably Shechem (cf. 289:22)". We will have more to say on the home of Labaya momentarily.

Gezer is located on the eastern edge of the plain of Sharon, on the important road leading from Joppa (Iapu? in letter 138:6) to Jerusalem. As such it was fortified by Solomon as the last line of defense against an attack from the west. We naturally expect that Jeroboam would invest Gezer at or around the same time he was threatening Megiddo. In fact, he had already conquered the city. Three letters from Labaya to the pharaoh have been preserved in the Amarna archives - numbers 252-254. In all of them Labaya protests his innocence against accusations of insurrection, claims he has been misrepresented, and vows his loyalty to pharaoh. In two of them he acknowledges only one possible "crime". He has captured Gezer. We quote from letter 252 as representative.

> [To the kin]g, [my lord a]nd my [sun] (shimshi).
> Thus saith [L]abaja, thy [servan]t and the d[ust] [whereon thou] treadest:
> [A]t the feet of the ki[ng], my [lo]rd,
> seven times and seven times I fall down.
> [I h]ave heard the words [whi]ch the king, my lord, has [wr]itten to m[e] [u]pon a tablet.
> [Ve]ri[ly], I am a servant of the king [as m]y [fathe]r and [g]randfa[th]er, servants of [th]e ki[n]g fo[r]me[r]ly were
> And I [have n]o[t] committed a crime nor have I sinned. This is my crime, an[d] this is my [s]in that *I have entered Gazri*.
> Thus I say: May the king be gracious to us. For, verily, now I have no other intention than to serve the king
> and to all that [the ki]ng [ha]s said I [ha]ve hearkened (253:1-31) [italics added]

The military siege of Megiddo and the capture of Gezer are entirely consistent with our thesis that Labaya and Jeroboam are the same person. But there are yet other indications that we are on the right track.

We have noted in passing the association of Labaya with the city of Shechem. Authorities on the Amarna documents are unanimous that this city was Labaya's home, based in part on a letter from a Jerusalem army commandant to the Egyptian pharaoh reporting the situation in Palestine.

> Labaja and the land of Shakmi have given (all) to the Habiru (289:22-24)

Shechem, located about thirty miles north of Jerusalem, near Mt. Ephraim, was the capital of Jeroboam's northern kingdom from the beginning of its existence.

> Then Jeroboam fortified Shechem in the hill country of Ephraim and lived there. From there he went up and built up Peniel. 1 Kings 12:25

The extent of Jeroboam's influence is not clearly spelt out in the Jewish literature, but since he set up shrines in Bethel and Dan, we can assume he "ruled over" territory from a line just north of Jerusalem to somewhere near lake Huleh. The fact that he "built up Peniel" tells us that his influence extended east of the Jordan to the region of Gilead. This is precisely the territory over which Labaya held sway. According to Albright:

> By one means or another Labaya was able to extend his control from the Mediterranean to the hills of Gilead and from the plain of Esdraelon to the frontiers of Jerusalem. Milkilu of Gezer and Tagu of Gath in Sharon were more or less faithful allies of his, and he kept the princes of Megiddo and Jerusalem in a perpetual state of apprehension. A son of his (?), Mut-Ba'al, became chief of Pella on the eastern side of Jordan south of Beth-shan. (CAH 2nd II part 2 p. 116)

It is interesting that in the Jewish historical books and in the Amarna letters alike, the two rulers, Jeroboam and Labaya , were associated uniquely with two cities which might legitimately be called "theirs", while "controlling" a larger territory which contained numerous other cities. Thus Jeroboam is associated particularly with Shechem and Peniel. And Labaya, in an otherwise confusing passage in one of his letters, declares:

> When I was hard pressed, on that very day *my two cities* were conquered (252:20-22) [italics added]

Geographically, the parallels between the rebel leaders Labaya and Jeraboam are nothing short of remarkable. This leaves us only to compare the two men on a personal level.

Personal Background.

Jeroboam, we know, was a commoner, the son of an otherwise unknown Israelite named Nabat. Solomon, recognizing his potential, had elevated him to a position "in charge of the whole labor force of the house of Joseph" (1 Ki. 11:28) A falling out between the two led to Jeroboam's

flight to Egypt, a (possible) subsequent marriage to the pharaoh's sister-in-law, and to his eventual rebellion. The biblical literature glosses over the details of his personal life.

> Jeroboam son of Nebat rebelled against the king. He was one of Solomon's officials, and Ephraimite from Zeredah, and his mother was a widow named Zeruah. 1 Ki. 11:26

That the two individuals, Jeroboam and Labaya, were both rebellious in nature goes without saying. In character they are strikingly similar. Both apparently had humble beginnings yet both exude a confidence/arrogance which suggests they were politically well connected. So Albright writes:

> That the latter's (Labaya's) beginnings were insignificant also appears in one of the earliest letters from him to pharaoh ... The truculence of Labaya's tone in writing to the court contrasts oddly with the grovelling subservience of most Palestinian chieftains. (CAH 2nd II part 2 p. 115)

It is not only Labaya's untoward confidence vis-a-vis his Egyptian overlord, and the lenient treatment he is accorded, that suggest he is somehow related to the pharaoh. In one of his letters to the Egyptian king Labaya mentions that the king has enquired regarding the well being of either his son (Mercer), or his father-in-law (Albright).[18] In the same letter he makes a statement that translators construe as a declaration of absolute loyalty:

> In case the king should write for my wife would I refuse her? (254:38-40)

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[18]The Akkadian word which describes this person is particularly troublesome to transcribe. Mercer translates it "my son" and adds the following note which summarizes other opinion: " ^{m}DUMU.MU-ia (also line 33): an unparalleled personal name, Dumuya (Knudtzon, Oppenheim, Seux), does not seem likely, and mi-mu-ia, "my father-in-law" (Albright, CAH 2/2, p. 115, n. 7), must be rejected on grounds of grammar (genitive expected in line 31) and especially paleography (the DUMU- and I-signs are quite distinct, and in both instances the sign in question is clearly the former). In view of ^{m}DUMU-a-ia at contemporary Taanach, most probably Binaya (Glotz, BASOR 204 (1971) p. 20), the same name may occur here. However, the apparent association of ideas - delivery of a son, readiness to deliver a wife - favors a common noun and a display of provincial learning: Sumerian DUMU.MU, "my son," plus Akkadian pronomninal suffix." (*The Tell-El-Amarna Tablets*, p. 308 note 4) It should be noted that the interpretation of Knudtzon and others to transcribe as a personal name "Dumuya" may not be essentially different than Mercer's opinion, since Dumuya in context is clearly a close relation of Labaya.

Admittedly this could be mere hyperbole, a gesture of absolute allegiance to the Egyptian king. But there may be a deeper meaning behind the words, a suggestion that pharaoh is acquainted with Labaya's wife and an acknowledgment that occasion might arise where she would be summoned (back) to Egypt. This would imply for her an Egyptian origin.

Taken at face value these political, geographical and personal parallels between Jeroboam and Labaya at minimum allow for the possibility that the two names relate to the same person. And considering that they have been placed independently in precisely the same historical time frame, one by a Jewish chronology generally accepted by scholars, the other by the Berlin genealogy chronology which has proved reliable in our earlier revisions, all but seals that conclusion. Only the difference in name seems problematic. If indeed there is a difference.

Labaya's Name.

It is the opinion of most scholars that the name of Jeroboam means "the people ('am) are multiplied/great (yarab)", a rather unusual name to assign to a young child to say least. Albright, in an attempt to circumvent this criticism, broadened both the verbal and nominal aspects of the name and understood it to mean "the family is extended," but his argument is unconvincing.[19] Problems with this stream of interpretation caused many critical interpreters to take *'am* as a reference to an otherwise unknown pagan god and translate "(the god) 'am is great". The point of this discussion is simply to call in question whether this was indeed Jeroboam's true name.

The similarity (both in sound and in meaning) of the name Jeroboam to that of his southern counterpart Rehoboam, the latter name taken by some to be that king's throne name (cf. Albright in AASOR 21-22 (1943), 67) suggested to a few scholars that the name Jeroboam may have been adopted as a parody on the name of Rehoboam, what one commentator has called "a defiant alias" on the part of the rebel king.[20] If so then Jeroboam's birth name has been lost in the process.

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[19]AJSL 38 (1922) 140ff.
[20]Montgomery, *The Book of Kings* ICC 248

A variant of the last hypothesis is the notion that the name of Rehoboam's rival has been supplied by scribes in the process of editing and transmitting the name over the centuries, until the final drafts of the books of *Kings* and *Chronicles* were composed during and immediately following the Exile. The fact that another king of identical name, Jeroboam II, ruled over Israel in that interim, suggests the possibility that the original name of Rehoboam's rival may have been assimilated to that of the later king in the process of transmission, a possibility all the more likely if the original name was vocally similar to that of the later king. The alteration may have been intentional.[21]

This preamble leads us to speculate on the three most reasonable explanations for the difference in name between Jeroboam and Labaya.

In the first place it is possible that the rebel "king" had several names, and that Labaya and Jeroboam are two among these. It is possible that one was Jeroboam's birth name and one a variation of a "throne name". Which is which is immaterial. This alternative name thesis is precisely what Velikovsky, in his *Ages in Chaos*, proposed to explain why biblical personal names do not appear in the Amarna literature.

> The kings of Jerusalem, as well as the kings of Samaria and Damascus also had more than one name. Five different names for Solomon are preserved. King Hezekiah of Jerusalem had nine names. In view of this practice, there is only a limited chance of finding in the el-Amarna letters the names of the kings of Palestine as we know them from the Scriptures. (Ages in Chaos, p. 234)

Alternatively we could assume that Labaya, the original name (but see below), was altered somehow during the course of transmission, perhaps deliberately changed by scribes to mimic the name of his rival Rehoboam, perhaps intentionally or unintentionally assimilated to the name of the

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[21]It has not gone unnoticed that the only two sons of Jeroboam mentioned in the biblical text, Nadab and Abijah have names similar to the two sons of the patriarch Aaron (Nadab and Abihu). At least a dozen other parallels between Aaron and Jeroboam I, including their worship of a "golden calf" idol, exist in the Hebrew Bible. Cf. Moses Aberbach and Leivy Smolar, "Aaron, Jeroboam, and the Golden Calves," JBL 86 (1967) 127-140. These close parallels suggests to some scholars that scribes have altered the Jeroboam story in at least minor ways in order to draw attention to the similarity between the idolatry of these two important biblical figures. If so then the likelihood of some comparison between the idolatrous 2nd Jeroboam and the idolatrous founder of the northern "kingdom" is also possible.

later Jeroboam, so that in time Labaya became Jeroboam in biblical literature. This is particularly likely because the names, as they now stand, bear a distinct resemblance to one another.

This resemblance, yet to be explained, suggests yet a third possibility, somewhat disguised by the form taken by the two names in transliterations of those names in English translation. The two names properly transliterated may actually be variant orthographic forms of a single name. A word of explanation is clearly in order.

It is the unanimous opinion of translators and commentators of the Amarna texts that the name of Labaya is derived from lb' (lamed, beth, aleph or lamed, beth, yod) the Semitic word for "lion". But the letters which denote this name were written in cuneiform Akkadian, leaving the transliteration less than clear and the original pronunciation of the name even more uncertain. As it stands, if the spelling lb' is correct and if the suffix "ia" is a reference to the Jewish god Yahweh, then the name means something like "Yahweh is a lion". [If "ia" refers to some other Semitic phonetic element then some other word play on "lion" is intended, though it is not clear precisely what that would be.]

But this interpretation is not so certain as we are led to believe by the unanimity among scholars. The Akkadian writing of the name in the Amarna documents typically consists of four cunieform characters, la-ab-a-ia. It is assumed by scholars that none of these elements is written ideographically.[22] It is further assumed that the third consonant in the name, if indeed there is one, is either an aleph or a yod. That there was a phonetic element following the "b" seems to be indicated in the writing by the presence of an "a", which leaves open the possibility that it represent only a vowel sound, or the vowel indicator "he", or even the gutteral 'ayin, rather than an 'aleph. This leaves the interpretation of the name an open question. We cannot even say for sure whether the suffixed element "ia" was actually sounded *at the end* of the name. If it refers to the divine name Yahweh, as we suspect, the possibility remains that it

[22]It is possible that the phoneme "la" represents the ideograph "LA " which means "abundance, plentitude" and that the signs following represent a verbal element or nominal complement which contribute to the meaning of the name. We recall that the name of Jeraboam is conjectured to mean "the people are abundant, many".

might have been sounded initially. At least once in the Amarna correspondence the name of Milkilu ("the god (ilu) is king (melek)") is written in reverse order (Ilumelek), leaving open the question how this name was actually pronounced. Thus Lab'aya might be a written representation of Yalaba' without regard to the meaning of the name. Labaya's Egyptian overlords would pronounce the name Yaraba' (Jeraba'), there being no "l" sound (or phoneme) in the Egyptian syllabary. Assuming the biblical spelling is the end result of a lengthy oral tradition, we can see how the name Jeroboam might arise. The names are practically identical.

We have encountered this type of problem before. In the first book of our series we discussed how the name of an Egyptian rebel, likely named Terek, became Tarku in Akkadian texts, Tirhakah in the biblical texts and has come to be transcribed Takeloth or Takeroth by Egyptologists. Names are fluid. In the course of time they change, usually maintaining key consonantal elements, but at times mutating with the addition of superfluous and at times meaningless glide elements.

Regardless of which explanation ultimately holds for the name of the rebel Labaya, we are confident in the identification we have made. But we anticipate at least one critical objection to our thesis. We have discussed the presence of Jeroboam in the Amarna texts. But what about Rehoboam?

Rehoboam.

The Amarna archives contain at least six lengthy letters (nos. 285-90) forthcoming from Jerusalem, all written by a single individual named Abdi-Hiba, and all complaining about the aggression of Labaya, though not always naming the aggressor. In his letters Abdi-Hiba laments that "the lands of the king, my lord, are lost" (286:22,23) and that "Ilimilku has devastated the whole land of the king" (286:36,37). Ilimilku, the editors note, "is to be identified with Milkilu" the author of several letters to the king and an ally of Libaya, apparently installed as "mayor" of Gezer after its conquest by the latter. Abdi-Hiba complains that further losses are imminent. He asks for military assistance.

> "The lands of the king are going to ruin." (But) you do not listen to me.
> All regents are lost; there remains not a regent to the king, the lord.
> Let the king turn his attention to the archers
> so that archers of the king, my lord, will go forth.
> No lands of the king remain.
> The Habiru plunder all lands of the king. (286:49-56)

In letter 289 he is more specific on the origins of the threat. The Philistine towns around Gezer are assisting the rebels.

> Verily, the land of [G]azri, the land of Asqaluna, and the city of of Lakisi
> have given them food, oil and all (their) needs (mâhziramu)
> So, let the king provide archers.
> Let him send archers against the people who commit evil against the king, my lord...
> Verily this is the deed of Milkili and the deed of Labaja
> which have given the land of the king to the Habiru . (289:14-19)

It is assumed by all commentators on the Amarna letters that Abdi-Hiba writes these letters in his capacity as the king of Jerusalem. If so then he must be Rehoboam and we are confronted with the identical problem we had with Labaya, namely, the difference of name. Assuming that the scholars are correct we can do no better than quote Velikovsky's opinion on the matter, in defense of his theory that Abdi-Hiba is another name for Jehoshophat, king of Judah during the reign of Ahab. At minimum these comments contribute to our discussion regarding the variant names of Labaya/Jeroboam.

> The name of the king of Jerusalem of the el-Amarna letters is read Abdi-Hiba. However, the same characters, if regarded as ideographic, permit another reading; at first it was proposed to read it Ebed-Tov ("The Good Servant" in Hebrew), and then Puti-Hiba; others read it Aradhepa or Arthahepa. From this fact we see that names written in cuneiform may be read in many ways, and the reading Abdi-Hiba is only one surmise among a number of others. It would appear that the original reading Ebed-Tov is preferable. (Ages In Chaos, pp. 235-236)

Velikovsky goes on to suggest that this lauditory epithet "The Good Servant" may have been another of the names of Jehoshophat. The argument, regardless of its strength, may well be applied to Rehoboam, the name "good servant" arising in order to contrast his behavior with that

of his evil northern counterpart. Having said that, we proceed to argue otherwise.

In our opinion Abdi-Hiba is not Rehoboam, the king of Jerusalem. These letters certainly date years after the conquest of Jerusalem by Shishak. Rehoboam remained king in the aftermath of that devastating defeat, but we assume that an Egyptian garrison was left behind to administer Egyptian interests in the area. Abdi-Hiba in these letters is clearly an agent acting on behalf of the Egyptian pharaoh, commissioned to receive and transmit tribute from Rehoboam to the Egyptian treasury. Rehoboam remains in the background and apparently has authored no letters, at least none that are extant. We are not guessing. Abdi-Hiba. clearly articulates his position.

> Verily, I am not a regent;
> I am an officer of the king, my lord.
> Behold, I am a shepherd of the king,
> and one who bears tribute of the king (288:9-12)

All of these references to "king" appear to refer to the Egyptian pharaoh. Nowhere in the letters does Abdi-Hiba refer to himself as king of Jerusalem.

> Let the king listen to Abdi-Hiba, thy servant, and send archers
> that they may again restore the land of the king to the king. (289:19-21)

In other contexts Abdi-Hiba speaks of his appointment to office at the initiative of the pharaoh, and makes it clear that his is not an inherited office, passed down from generation to generation as was the case with "kingship" in Israel.

> Neither my father nor my mother, (but) the mighty hand of the king
> [has] se[t me] in the house [of my father] (288:13-15)
> [Ve]rily, this land of Urusalim, neither my father nor m[y] mother has given (it)
> to me; the [mi]ghty hand [of the king] gave (it) to me. (287:25-28)

The phrase "of my father" is added by the editors to the quote from letter 288. It almost certainly was not the original wording in the elided portion of text, which may well have referred to Rehoboam by name.

Addendum

At minimum the preceding discussion allows the possibility that our theory regarding the Berlin genealogy chronology is correct. If we are wrong then even the staunchest critic of our revision must admit that we have been most fortunate. The Berlin chronology for the late 18th dynasty was nothing more than an extrapolation of the genealogy already applied successfully to the 7th century revisions of our earlier book and to the 8th century revisions proposed in the first chapter of the present book. This extension compelled us to place the Amarna correspondence roughly in the time frame 930-910 B.C., the time of the division of the Israelite kingdom. We had no choice in the matter.

Consequently, had we not already read the Amarna letters from the Palestine region, we would have expected to find in them reference to a rebel king, residing in Shechem and in control of the entire region north of that city, ranging from the Mediterranean to the trans-Jordan. We would have expected that he would be in conflict with Megiddo and Gezer and Jerusalem, the cities in the southern region fortified by Solomon several decades earlier and now held by Rehoboam. We would have anticipated that Egypt, the former sanctuary of the rebel king, and more recently the conqueror of Jerusalem, would have an established presence in the area. And we could have predicted that Egypt would be siding with the rebel king, considering his previous liason with the Egyptian court, even if we discounted the midrash contained in the Septuagint regarding a marriage between him and pharaoh's sister-in-law. We would have demanded that the city of Rehoboam be named Jerusalem and that ethnic references to the rebels, if any were given, would allude to them as Hebrews (Habiru). There is nothing in the Jewish historical books describing the actions of Jeroboam and Rehoboam that conflicts with the data in the Amarna letters, in spite of the fact that many dozens of letters are forthcoming from the region of Palestine. The reader can believe, if he/she so chooses, that this is merely a fortunate coincidence. Or he can accept the ever increasing volume of data, by now filling well over 700 pages of closely reasoned text in two books and the beginning of a third, arguing for the accuracy of our revised chronology.

Chapter Three
Empires Collide – Contestants for Syrian Domination in the 9th/8th Centuries B.C.

The Contestants

In chapter one of the present book we reasoned that Ramses II must have ruled Egypt in the time frame 840-774 B.C. In chapter two we argued that the Amarna letters, at least those originating from the Palestine region, were written in the final decades of the 10th century B.C. (roughly 930-910). It follows that the kings of Egyptian dynasties 18 and 19 must have been contemporaries of the Jewish rulers of the united and divided kingdoms, whose lives are detailed in outline form in the historical books of the Hebrew Bible. They must also have lived and interacted with many of the powerful neo-Assyrian kings who dominated the trans Euphrates region during this time period, many of whom left for posterity detailed records of their dealings with the surrounding near eastern world. It should be possible, at least in theory, to synchronize the histories of these three dominant countries - Egypt, Israel and Assyria - particularly as they relate to the region of Lebanon/Syria, a land centrally located between, and therefore of considerable importance to, all three nations. The development of such a synchronized history lies far beyond the purview of this book. Let others take up the task. Our interest here is restricted to the most obvious synchronisms between significant events and prominent historical figures in the respective countries.

Yet a fourth nation enters the picture. Several times already we have had cause to mention the Hittites, the dominant Anatolian kingdom during the reigns of the 18th and 19th dynasty kings, unknown to modern historians until the discovery of the archives of several Hittite rulers early in the 20th century. Those archives, in conjunction with the Amarna letters, leave no doubt that Suppiluliumas, the most prominent king of the Hittite Empire period, was a contemporary of the terminal 18th dynasty kings, and that his successors Mursilis, Muwatallis and Hattusilis, were contestants for dominance in the Syrian region in the days of Seti and Ramses II of the 19th dynasty. As such these Empire kings are central to our discussion of the two centuries which concern us in this revision.

Our objective in this chapter is patent. All scholars accept the fact that the Empire Hittites were contemporaries of the (late) 18th and 19th dynasty Egyptian kings. When we moved the two Egyptian dynasties forward in time by well over four hundred years, we necessarily moved Suppiluliumas and his successors forward an equal number of years, into the 9th/8th centuries. This fact doubles our chances of confirming the accuracy of our 9th/8th century B.C. placement of Egyptian dynasties 18 and 19. We can now search for further evidence that *either* the 18th or 19th dynasty Egyptians, *or* the Empire Hittite successors of Suppiluliumas belong to the 9th/8th centuries. That will be our objective in the balance of this book. But first, as a necessary backdrop to our discussion, we need to produce king lists for Egypt, Assyria and Hatti for the time period in question. The chronology of Israel in the 9th and 8th centuries B.C. is generally well known and will not be reproduced here. Dates for these kings will be noted on an ad hoc basis.

Egypt

To date we have provided dates for most of the terminal 18th dynasty kings, having established provisional dates for Amenhotep III (964-928 B.C.), Amenhotep IV/Akhenaton (940-923), Tutankhamon (923-914) and Ay (918-914) (see table 5 on page 25, reproduced below as table 6).

We also reasoned that the 19th dynasty king Ramses II ruled the country from 840-774 B.C., in substantial agreement with the Berlin chronology. But it has no doubt occurred to critics of our revision that we are left with an embarrassingly long interval of 74 years between Ay and Ramses II in which to insert the reigns of Haremheb, Ramses I and Seti I, who are credited with 27, 2, and 10 years respectively by the traditional history. How are we to account for the 35 superfluous years which fill the gap following Ay? Clearly we must either adjust our dates for Ramses II, lengthen the reign lengths of the kings Haremheb, Ramses I and/or Seti I, or assume an extended period of civil unrest during which no king ruled in Egypt (an interregnum). Either that or we must adjust our previously established chronology for the kings of the Amarna age. Whatever we do, we must be guided by the Berlin genealogy, which indicates that Horemheb was ruling Egypt at least as late as 885 B.C., while Seti

assumed power at least as early as 869 B.C. In the end we will adopt, in part, three of the four suggested remedies.

The dates for Ramses II are sacrosanct and cannot be changed appreciably. To set them aside would mean that we cannot rely on the chronology imbedded in the Berlin and Ashakhet stelae. In the discussion below we argue instead that the combined reigns of Akhenaten, Smenkhare, Tutankhamon and Ay may have extended to a slightly later date than suggested earlier, perhaps as much as ten years later. We also argue that the deaths of Ay and Tutankhamon may have been followed by a brief period of civil unrest. And finally, we increase the reign length of Seti considerably beyond the dozen years assigned to him by modern scholars. Our discussion will result in the following revised dates for the terminal 18th dynasty kings and the initial two kings of the 19th dynasty.

Table 6: Proposed Dates for the Late 18th Dynasty Kings, including Ramses I & Seti I of the 19th Dynasty

Name	Regnal Years
Amenhotep III	964-928
Amenhotep IV (co-regent with his father)	940-928
Amenhotep IV	928-923
Smenkhkare	923-920
Tutankhamun	923-914
Ay	918-904
Interregnum (Haremheb as general)	904-897
Haremheb (as king)	897-870
Ramses I	870-869
Seti I	869-840

Our defense of these dates proceeds upward from the reign of Seti.

Seti I

Seti I is widely acknowledged as one of the great builders in Egyptian history. The vast number of monuments and constructions credited to his reign, and the number and frequency of his military enterprises, all but demand that he be assigned more than the dozen years accorded him by

modern historians. In fact, earlier generations of Egyptologists did credit this king with a reign length at least double this number, and the incontestible fact that he died just short of his first heb seb (30 year) jubilee festival, based on at least one monument long known to scholars, makes us wonder at the strained chronology which insists on maintaining for him the fiction of a short reign. As early as the turn of the 20th century, Breasted argued in his *Ancient Records* that ...

> Seti's reign may have been considerably longer than is usually attributed to him. He was about to celebrate his jubilee when he died, having left an obelisk unfinished, so that it was completed by his son, Ramses II. If his father reigned two and a half years, Seti's jubilee might have fallen in the middle of his twenty-eighth year. But as he did not live to complete the obelisk and celebrate the jubilee, he may have died a few years before the jubilee, after a reign of over twenty years. BAR III 131 (p. 67).

The logic of Breasted's calculations escapes me. If he is suggesting that Seti included the two years of his father's rule as part of his own we should point out that there is no evidence of this practice. The all but completed jubilee monument, taken at face value, establishes the fact that Seti reigned into his own 29th year. Scholars are agreed that Ramses II used the stele, modified only slightly, to commemorate his 30th year in office. Why would Seti have intended it to be used otherwise?

The twenty nine years brings the date for Seti's inauguration back to the year 869 B.C., in full agreement with our interpretation of the Berlin chronology, which places him in office in the years 869 and 853 B.C.

Ramses I

The two years assigned to Seti's father Ramses I in the revised history (870-869 B.C.) is consistent with the monuments and with the traditional overview of this king's very brief reign. We say no more about Ramses I in this revision.

Horemheb

If we assign to Horemheb the 27 years credited to him by the traditional history (a somewhat arbitrary figure based as much on the "inscription of Mes" (see below) as on any monuments), and if we assume his reign followed immediately the death of his predecessor, then his kingship would span the years 914-887 B.C., leaving a gap of 17 years between his death and the beginning of the reign of Ramses I. This state of affairs has no support from the monuments which indicate that Horemheb's tenure as pharaoh terminated the year Ramses I declared his kingship. In the revised history that would be 870 B.C. The 17 years unaccounted for must precede, rather than follow, his kingship, which means that his reign officially began in 597 B.C., or slightly earlier if he reigned longer than 27 years.

We know from multiple strands of evidence that Horemheb functioned as a general in command of the Egyptian army long before he became pharaoh. That military career dates as far back as the reign of Akhenaten. Through the reign of Ay his power increased sufficiently that he became, in his own words, the *de facto* ruler of Egypt. In a lengthy document known as his "Coronation Inscription" (BAR III 22-32) he recounts his rise to power from his youth, through his career at court, to his ultimate coronation. The wording of the text allows for the possibility, assumed here, that for some period of time following the death of his predecessor, and before he assumed the status of pharaoh, he ruled Egypt as an administrator. How else are we to interpret his declaration that after a period of time as an appointed deputy of an unnamed king (either Ay or Tutankhamon [see below]) there existed another phase of his career when

> Behold, he administered the Two Lands during a period of many years; (when) there reported [to him] -- there [bowed down] to him the council in obeisance at the front of the palace, there came to him the chiefs of the Nine Bows, South as well as North; their hands were spread out in his presence, they offered praise to his face as (to) a god.... When he came, the fear of him was great in the sight of the people; prosperity and health were besought for him; he was greeted : "Father of the Two Lands, excellent counsel of divine gift ... BAR III 26

There follows in the text a lengthy description of his coronation, said to have taken place only after "many days had passed by" during which he

functioned as "chief and hereditary prince". There is no suggestion that his coronation immediately followed the death of an existing sovereign. We assume therefore that Horemheb ruled the country for “many days” before assuming pharaonic status. If he became pharaoh in 897 B.C., or perhaps a few years earlier, then we hazzard a guess that this "protectorate" might have begun as early as 904 B.C. It was certainly in force around the year 900 B.C. when the Hebrew Bible records an aborted attack by "Zerah the Cushite" on the fiefdom of Asa, the son of the Israelite rebel Jeroboam. We have previously suggested that this Zerah should be identified as Horemheb during the later's ascendancy, before he was widely recognized as pharaoh within Egypt.

Before proceeding we underscore the fact that this assumed interregnum is not an essential element in our argument. It is possible that Horemheb's reign actually extended seven or 8 years beyond the 27 years credited to him, and that he began ruling Egypt *de lecto* as early as 904 B.C. It is for other reasons that we document the high esteem in which this "army general" was held long before he became pharaoh. At least one monument preserves a memory of his extended "kingship" and since the text of this inscription also confirms one other aspect of our revised chronology it deserves mention in passing.

We have suggested, based largely on the Berlin chronology, that Amenhotep III ruled Egypt from 964-928 B.C. His successors, beginning with the sole reign of the "heretic" Akhenaton, were not highly regarded by the Egyptian populace, and the country was thrown into a period of confusion. Horemheb's rise to power occurred largely in this chaotic time frame. It appears that he was first appointed to office under Akhenaton after the later's move to Akhetaten (Amarna) following the death of Amenhotep III, thus in 928 B.C. If our relative chronology is correct Horemheb's career spanned the years from 928 to 870. He would have died in his 59th year of "ruling" in Egypt. It is therefore significant that at least one monument mentions that year in its dateline.

Since first published in 1901, a document popularly known as the "Inscription of Mes" has intrigued scholars, not only as one of the longest and most comprehensive legal documents ever unearthed in Egypt, but for the insights it provides into the chronology of the 18th and 19th

dynasties. The inscription documents a legal battle over the ownership of land which lasted for over a century, the latest phase of which took place during the reign of Ramses II. As in all legal manoeuvers, the various stages of the litigation process are historically referenced, and one phase in particular is clearly indexed to the "year 59 under the Majesty of King Horemheb".[23] Loret, who first published the document, "proposed as an explanation of the high date" that "Horemheb reckoned the reigns of his heretic predecessors as belonging to himself", an understanding shared by Egyptologists generally.[24] They are undoubtedly correct.[25]

The significance of this document for our revised chronology is obvious. Our relative chronology assumed as its basis the reliability of the genealogy provided by the Berlin and Ashakhet stelae. The fact that the absolute chronology derived from it agrees with the "Inscription of Mes" so precisely, confirming as it does that Horemheb lived into his 59th year in office, is at least suggestive of the fact that we are on the right track. The combined "reigns" of Horemheb (ignoring Akhenaten, Ay and Tutankhamon), Ramses I, and Seti, precisely fill the time frame from the death of Amenhotep III to the advent of Ramses II.[26] There are no superfluous years.

Akhenaten, Smenkhkare, Tutankhamon & Ay

The observant reader will have noticed that we have revised our earlier dating of Ay, assuming he was in fact the pharaoh who preceded Horemheb. In the above discussion we have suggested that either

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[23] See Alan Gardiner, "The Inscription of Mes: A Contribution to the Study of Egyptian Judicial Procedure", Vol 4 in Kurt Sethe ed. *Untersuchungen zur Geschichte und Altertumskunde Aegyptens*, p 11

[24]Thus Gardiner (op cit p. 22) adds the comment "This is undoubtedly the correct view". The original remarks of Loret are found in ZAW 39 (1901) 4.

[25] The so-called "Table of Abydos", a king list contructed by Seti I, cites Horemheb as the immediate successor to Amenhotep III, omitting mention of the reigns of Akhenaton, Tutankhamon and Ay, providing support for Loret's interpretation.

[26]Critics might argue that the chronology of the traditional history also allows for precisely 59 years between the Amenhotep III (1405-1367) and the death of Horemheb (1335-1308?). But this chronology was constructed with the "Inscription of Mes" dateline specifically in mind. (We note especially the question mark affixed to the date for Horemheb's death, supposedly in 1308 B.C., reproduced from Gardiner's *Egypt of the Pharaohs*.) Not so with the revised history chronology which followed necessarily the genealogy of the Berlin stele.

1) Horemheb began to rule Egypt around the year 905/904 B.C. or that
2) his predecessor died around that time and an interregnum followed for several years until Horemheb officially declared his kingship. Previously, based largely on the Berlin chronology, we had suggested that Ay, who presumably preceded Horemheb in office, ruled from 918-914 B.C. How do we justify this "change of mind".

In fact, we have not changed our mind. The dates provided earlier were based on the reign lengths of the 18th dynasty kings Tutankhamon and Ay provided by the traditional history. At the time we were concerned only to explain how Ay might have been pharaoh in 917 B.C., in keeping with a strict interpretation of the Berlin chronology. But the reign lengths we used for Ay and Tutankhamon are at best an educated guess on the part of scholars, as is the assumed order of the two kings. The traditional history considers that the young king Tutankhamon was dethroned by the elderly Ay, who in time murdered his young rival. But this is pure guesswork on the part of scholars. There is no evidence of this in the monuments. We could argue instead that when Ahkenaton died, Tutankhamon was still an infant, and that Ay ruled during that infancy, eventually yielding power to Tutankhamon. In that case his reign began at the death of Ahkenaten in 923 B.C. and may well have extended half a dozen years beyond the four years typically assigned to him, say from 923-914 B.C.. Tutankhamon's nine years would occupy the time frame from 914-905 B.C. Considering the chaotic conditions that prevailed at the time, as suggested by the odd inscription that has been preserved from this period, we simply do not know precisely what transpired.

Alternatively, we might lower the 917 date referenced to Ay in the Berlin genealogy. Throughout we have allowed the possibility of minor local variations in the dates based on that monument, though we have had no reason to change a single number. It is possible that Akhenaton began and ended his kingship at dates slightly lower than those suggested, thus reducing the length of time he ruled jointly with his father. If so then it is possible that Ay did not begin his reign until around 915, or even a year or two later. And if he did, and if he and Tutankhamon ruled in succession, then their combined reigns would extend as late as 900 B.C., when Horemheb's reign might have began. Under these conditions we could discount the hypothetical "interregnum" discussed earlier.

We leave the matter there. One point only should be borne in mind from these considerations. The reign of Ay and Tutankhamon may well have extended to the year 900 B.C., about 15 years beyond what was previously proposed. This implies that the dates for the Amarna correspondence (though not necessarily the letters from Palestine) should be lowered somewhat from those previously suggested. Those documents may date as late as the beginning of the 9th century.[27] The Amarna age, as adjusted, must lie in the years 930-900 B.C., and perhaps a few years beyond. And we need not assume, as is done in the traditional history, that Akhetaten (Amarna), the site of Akhenaten's reforms, was destroyed by Ay. It may not have been abandoned until a few years into the reign of Horemheb. Its destruction may have awaited the onset of the reigns of Seti and Ramses II.

Assyria

Since the end of the 19th century scholars have more or less agreed on absolute dates for those Assyrian kings who ruled during the 9th through 7th centuries B.C. This agreement is based largely on a few references to individual Assyrian kings in well defined historical contexts in the Hebrew Bible, augmented by detailed lists of kings, with regnal lengths included, preserved on cuneiform tablets excavated in the Assyrian homeland (the so-called Assyrian king lists). Additionally the Assyrian Eponym Canon provides a year by year accounting of the reign of each Assyrian king, referenced to the name of a leading official (limmu) in the country. The Canon extends from the end of the 10th century through the end of the 8th century, thus providing a reliable *relative* chronology for this important time frame. Variations do exist in the *absolute* dates assigned individual kings by the current generation of scholars, but these differ from the dates provided below by at most a few years. With that in mind we reproduce one such modern list, with dates sufficiently accurate for the purposes we have in mind.

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[27]It is generally assumed by scholars that Ay was the party responsible for the destruction of the site of Akhetaton (Tel el-Amarna). We believe instead that Horemheb should be viewed as the party responsible, and that the event took place very early after he became pharaoh. At least one of the Amarna letters, that from the Hittite king Suppiluliuma, is likely addressed to him.

Table 7: Neo-Assyrian Kings of the 9th and 8th Centuries B.C.

Adad Nirari II	909-889	
Tukulti Ninurta II	888-884	son of Adad Nirari II
Ashurnasirpal II	883-859	son of Tukulti Ninurta II
Shalmaneser III	858-824	son of Ashurnasirpal II
Shamshi Adad V	823-810	son of Shalmaneser III
Shammuramat (Shamiram)	809-792	wife of Shamshi Adad V during infancy of their son
Adad Nirari III	791-782	son of Shammuramat & ShamshiAdad V
Shalmaneser IV	781-772	son of Adad Nirari III
Ashur Dan III	771-764	
Adad Nirari IV	763-754	
AshurNirari V	753-746	
TiglathPileser III	745-722	
Shalmaneser V	726-722	
Sargon II (722-705)	722-705	
Sennacherib (705-681)	705-681	son of Sargon II
Esarhaddon (680-669)	680-669	son of Sennacherib, conquered Egypt in 670 B.C.

Hatti

The situation for the Hittite kings is quite different from that which prevailed in Assyria. On the one hand, because Hittite chronology is inextricably linked to that of Egypt, the commonly accepted dates for its Empire kings must be radically lowered to conform to the changes in Egyptian chronology introduced in our earlier chapters. On the other hand

the reign lengths of the Hittite kings is not so well established as those for Assyrian (and Egyptian) kings, and must, for the most part, be surmised from other considerations.

It is well known that three of the Empire kings of Hatti - Muwatallis, Mursillis and Hattusilis - were contemporaries of the Egyptian king Ramses II, whose dates we have moved forward 450 years. It follows that the accepted Hittite chronology must move in step an identical number of years. We reproduce below the table of Hittite kings from Gurney's Pelican classic on *The Hittites,* followed by a second column with all traditional dates reduced by 450 years. The resultant chronology (in the third column) serves as a basis for the minor adjustments made in the final column. An explanation of these refined dates follows the table.

Table 8: Hittite Kings of the Empire Period

Name	Traditional Dates	Dates Reduced 450 yrs.	Dates Further Adjusted
Suppiluliumas	1375-1335	925-885	908-858
Arnuwandas III	1335-1334	885-884	858-857
Mursilis II	1334-1306	884-856	857-836
Muwatallis	1306-1282	856-832	846-832
Urhi-Teshub (Mursilis III)	1282-1275	832-825	832-825
Hattusilis III	1275-1250	825-800	825-800
Tudhaliyas IV	1250-1220	800-770	800-775
Arnuwandas IV	1220-1200	770-750	775-765
Suppiluliumas II	?	?	765-760

In the traditional history the final years of the reign of Muwatallis, the whole of the reign of Urhi-Teshub, and much of the reign of Hattusilis III are inextricably linked to the years of the Egyptian pharaoh Ramses II. Documents from both Hittite and Egyptian archives confirm that Muwatallis fought a battle against Ramses at Kadesh on the Orontes in

Ramses' 5th year. Egyptian documents also attest a peace treaty forged between Hattusilis and Ramses in Ramses' 21st year, and a marriage alliance between Ramses and a daughter of Hattusilis in Ramses' 34th year. Since we have dated the beginning of Ramses' reign to the year 840 B.C., these three events must have taken place in the years 836, 820 and 807 B.C. respectively. Our refined chronology must ensure therefore that Muwatallis was on the throne in 836 B.C. and that the reign of Hattusilis spans the years 820 and 807. Accordingly we have left the reigns of Urhi-Teshub and Hattusilis unchanged from the reduced dates assigned them in column 2 and we have maintained the 832 B.C. date for the death of Muwatallis. Changes elsewhere in the king list, including a ten year reduction in the reign length of Muwatallis, and the assumption of an overlap in the reigns of Mursilis and Muwatallis, require some explanation.

It should be pointed out at the outset that the changes to the reign lengths of the first four and final three kings of the Hittite Empire can neither be confirmed nor denied. None of these kings, save Mursilis II, have left for posterity any documents which specify how long they actually reigned. Most have left no dated material of any sort. When traditional historians produced the dates in column one of our table they were, for the most part, making an educated guess as to the length of each king's reign. In modifying these reign lengths we are merely extending to ourselves this same privilege.

Traditional historians have correctly surmised that Suppiluliumas reigned for at least three or four decades, based in part on his legendary status and the accomplishments credited to him by subsequent generations, especially by his son Mursilis II. He died when plague ravaged the Hatti lands (see below). Scholars typically assign to him somewhere between 35-50 years. We assume the highest of these numbers although, as we explain below, our chronology can accommodate a reduction in that figure, lowering the beginning date for his reign by as much as ten to twelve years. On the other hand his son Arnuwandas III cannot have reigned for more than a few years before succumbing to the same plague that killed his father. Another son of Suppiluliumas, our Mursilis II, then replaced his brother as king. Fortunately the annals of this king have been preserved for us to read. One inscription in particular provides us with a

year by year accounting of Mursilis' military actions spanning the first ten years of his reign. Another group of inscriptions, less well preserved, can with difficulty be cross referenced to the "ten year annals" and appear to extend those annals a further ten to twelve years. Accordingly Albert Goetz, arguably the 20th century's foremost authority on the Hittite inscriptions, credits Mursilis with a reign of 22 years.[28] This figure may be excessive, though for convenience it is retained in the table 9 listing, where we have assumed that Mursilis ruled into his 22nd year.

The traditional history credits Muwatallis, son of Mursilis, with a reign of 24 years, but this is an approximation based on historical references contained in the inscriptions of his brother Hattusilis, who much later usurped the kingship from Urhi Teshup, the son of Muwatallis. Hattusilis claims to have served under Muwatallis in a quasi regal capacity defending the northern frontier of the country, then to have assisted his brother quell a revolt in Arzawa, and finally, to have commanded the Hittite army on behalf of Muwatallis in the battle of Kadesh against Ramses II in 836 B.C. In one reference Hattusilis claims to have served his brother for ten years, which we believe to be the full extent of that service. And since Muwatallis appears to have promoted his brother to his elevated status soon after he became Great King of Hatti we need not assume that he ruled for much longer than that time, though in table 9 we credit him with 14 years, admittedly only an educated guess. The 24 years assigned this king by the traditional history has little support.

We have no quarrel with the registered reign lengths of Urhi-Teshup (Mursilis III) and Hattusilis. And our modest reductions to the reign lengths of the successors of Hattusilis are made to accommodate our belief that the Hittite Empire ended around the year 760 B.C.

Only one aspect of the table 9 adjusted chronology requires further comment. We have somewhat arbitrarily assumed an overlap between the reigns of Mursilis and his son Muwatallis, an adjustment forced on us by

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[28]According to Goetze "The preserved parts of the annals of Murshilish justify the assumption that his reign covered more - and probably not much more - than twenty two years". (CAH II Part 2 p. 126-7) The operative word here is "assumption". Goetze admits that "What we possess of annals from the later years of Murshilish - it is unfortunately incomplete - does not relate any large-scale military operations anywhere." (p 125-6) During these years Mursilis apparently fought only defensive battles on the north of his realm.

other considerations. In the next section we present arguments that Suppiluliumas ruled in the approximate time frame 908-858 B.C. We are fairly certain of the year 858 for his death. We are also certain that the rule of Muwatallis extended through the year 836, the 5th year of Rames II. And we see no reason why he might not have ruled into Ramses 9th year as argued by the traditional history, thus placing his death in 832 B.C. If we are correct in these assumptions then there exist only 26 years available to accommodate the reigns of both Mursilis II and Muwatallis, to whom we have assigned 22 and 14 years respectively. Even if we reduce the reign of Muwatallis from 14 to 10 years, the combined reigns of these two kings exceed the years available to them. One solution to the problem is to assume the existence of a "co-regency", for which there is ample precedent in Hittite politics. We have already seen how Hattusilis acted as a regional king for upwards of ten years in support of his brother Muwatallis, who alone retained the title of "Great King of Hatti". Many Hittite kings assigned their near relatives, often their sons, suzerainty over key cities, with the title "king of [city name]". In the course of our research we will see that Assyrian annals frequently refer to rulers of Syrian city states as kings of Hatti or Hattini, most of whom are only regional kings. It should therefore be no major problem if we argue that Muwatallis ruled some portion of the Hittite Empire while Mursilis was alive, and that his achievements during this phase of his career might mistakenly be attributed by scholars to his sole reign. And in the case of Muwatallis and Mursilis (II) we can suggest a reason why Mursilis might even relinquish to his son many of the functions typically assigned to the "Great King", all this while he was still alive. The reason is manifest in several of his extant inscriptions. For much of his life beyond his tenth year *he was not well.*

The Illness of Mursilis

In the initial paragraphs of his ten year annals Mursilis mentions the recent demise of his father and brother resulting from a devastating plague that ravaged the Hittite Empire.

> Thus (speaks) My Sun Mursilis, Great King, King of Hatti, the Valiant, son of Suppiluliumas, Great King, the Valiant. Even before I sat on my father's throne, all the enemy lands were hostile to me. When my father became a god (i.e. died),

> my brother Arnuwandas sat on his father's throne. Afterwards, he too became ill. When the enemy lands heard Arnuwandas (was) ill, the enemy lands began to be hostile. CTH 61

In yet another document the young king identifies the plague as divine punishment for certain sins of commission and omission on the part of his father Suppiluliumas, sins which he then sets about to atone for. This inscription (of which multiple copies exist), popularly known as the "Plague Prayers of Mursilis", appears to date sometime after the king's tenth year, this based on internal considerations.[29] In it Mursilis describes the extensive loss of life inflicted on the Hittite nation by a plague which has lasted twenty years, beginning late in the reign of Suppiluliumas, and persisting through that of his brother Arnuwandas into his own. We suspect, though Mursilis does not clearly admit it, that he himself is now ill.

> Hattian Storm-god, my lord, and ye, Hattian gods, my lords! Mursilis, the great king, your servant, has sent me (with the order:) Go! To the Hattian Storm-god, my lord, and to the gods, my lords, speak as follows: What is this that ye have done? A plague ye have let into the land. The Hatti land has been cruelly afflicted by the plague. For twenty years now men have been dying in my father's days, in my brother's days, and in mine own since I have become the priest of the gods. When men are dying in the Hatti land like this, the plague is in no wise over. As for me, the agony of my heart and the anguish of my soul I cannot endure any more. .... Drive ye forth the plague from the Hatti land!" ANET 394-5

The prayer becomes more personal and more intensive as it continues. Near its end Mursilis pleads for his own life.

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[29] The fact that twenty years have passed since the plague first appeared in the Hatti land necessitates dating the stela sometime before the fifteeth year of Mursilis, probably closer to the tenth. The plague must have appeared five to ten years before the end of the reign of Suppiluliumas considering the sequence of actions by that king which are cited as its cause. It certainly does not date from the first ten years of Mursilis. Toward the end of his lament/prayer Mursilis acknowledges that "the protectorates beyond the frontier, (namely) the Mitanni land (and) the Arzawa land, each one has rebelled; they do not acknowledge the gods and have broken the oaths of the gods. They persist in acting maliciously against the Hatti land." Further on he notes that "those countries which belong to the Hatti land, (namely) the Kashkean country ... also the country of Arawanna, the country of Kalasma, the Lukka country, the country of Pitassa - these lands have also renounced the Sun-goddess of Arinna. They cast off their tributes and began to attack the Hatti land in their turn" ANET 396 This state of affairs differs markedly from that which prevails in the ten year annals.

> Hattian Storm-god, my lord, save my life! Let this plague abate in the Hatti land.

Loss of life was widespread - both human and animal populations were drastically reduced.

> What is this, O gods, that ye have done? A plague ye have let into the land. The Hatti land, all of it, is dying; so no one prepares sacrificial loaves and libations for you. The plowmen who used to work the fields of the god are dead; so no one works or reaps the fields of the god at all. The grinding women who used to make the sacrificial loaves for the gods are dead; so they do not make the sacrificial loaves any longer. From whatever corral (or) sheepfold they used to select the sacrifices of sheep and cattle, the cowherds and the shepherds are dead and the corral [and the sheepfold are empty] ANET 396

According to our makeshift chronology the plague lasted from about 865 to 845 B.C., into the twelfth year of Mursilis II. If the plague did not take the king's life, then we assume minimally that it did prompt him to elevate his son Muwatallis to assist him in defending the country. And soon thereafter Hattusilis, another of Mursilis' sons was also summoned to help.

Even if Mursilis survived the plague and lived out the balance of the twenty-two years credited to him by Goetze, he was hardly fit to govern the country. At least one other document suggests that about the same time he also suffered a stroke which permanently affected his speech and perhaps also his capacity to govern effectively.

> Thus spoke His Majesty Mursilis, the Great King. I rode to Til-Kunnu ... and suddenly a thunder-storm broke out, whereupon the storm god caused terrible thunder and I became afraid and the speech faded away in my mouth and the words rose up with some difficulty. These happenings I forgot completely. But as the years came and passed by, it happened that this matter repeatedly occurred in my dreams and the hand of god struck me during a dream and my mouth went askew. Houwink ten Cate, 1966 p.34

We leave the matter there. We are confident that our dates are approximately correct. Further corroboration is forthcoming as we relate the lives and actions of the Hittite kings to the activities of their contemporaries in Assyria.

Chapter 4
Hittite Synchronisms with the 9th/8th Centuries

Dark Ages & Ghost Cultures

When early historians mistakenly placed the 18th and 19th dynasties of Egypt in the 16th-13th centuries B.C. the ripple effects on the histories of multiple Aegean and Near Eastern civilizations were profound. The Empire kings of Hatti, the Minoan and Mycenean Greeks, multiple north Syrian confederacies including the north Syrian city states of Carchemish and Aleppo, as well as Ugarit and the cities of Amurru on the eastern Mediterranean coast, were all artificially thrust back in time to a period four to five hundred years before they actually flourished, all because their histories were enmeshed with the reigns of the 18th and 19th dynasty Egyptian kings. And the error did not stop there. The Balkans and a host of European countries north and east of the Black Sea suffered the identical distortion of their ancient history. A gap of around 450 years was created in the archaeological and cultural historical records at literally hundreds of sites in these and other contiguous locations throughout the near and middle east. So-called dark ages, lasting hundreds of years, appear mysteriously in cultures throughout much of this vast geographical area. Israel and Assyria alone (at least as far back as the 10th century) - and to a lesser degree southern Syria - escaped this gross historical distortion, saved by the meticulous and continuous annals maintained by the two key nations, whose histories are linked by the occasional synchronism in the public records.

This distortion of history has not gone unnoticed. On an ad hoc basis archaeologists and social historians have been dealing with it for over a hundred years. Revisionist historians have documented the error exhaustively and have repeatedly identified a faulty Egyptian chronology as its cause.

Nowhere is the error more obvious than in northern Syria, which acts as a buffer state between the Hatti land and Assyria, and was, at various times in the classical Hittite period, part of the extended Hittite empire. The

armies of multiple neo-Assyrian kings of the 9th and 8th centuries frequently visited the area with varying degrees of success, inevitably encountering city states allied with the Hittites, ruled by kings bearing names familiar from the Empire period of the nation.

According to O.R.Gurney, the British scholar who first popularized the Hittite situation to the English speaking world,

> in the south-eastern provinces of the Hittite Empire Hittite culture had a strange afterglow which lasted for no less than five centuries. Assyrian records continue to refer to Syria and the Taurus area as the 'Land of Hatti' and speak of kings bearing names like Sapalulme, Mutallu, Katuzili, and Lubarna (cf. Suppliluliumas, Muwatallis, Hattusilis/Kantuzzilis, and Labarna). *The Hittites* 39

From the outset of Hittite studies the possibility has never been entertained that these later occurrences of Hittite names, and the Hieroglyphic Luwian monuments so characteristic of these "land of Hatti" Syrian states, are nothing other than what they appear to be, evidence that the Hittite Empire was alive and well in the 9th/8th centuries. The term "neo-Hittite" was affixed to these "out of place" events and archaeological artifacts, and a theory was developed to explain the phenomenon as a "renaissance" of Hittite culture, an example of what we have termed “ghost cultures” in our division heading. Traditional historians had no other choice, since according to that history the Hittite civilization had ceased to exist around the year 1200 B.C., destroyed in a calamitous physical and social upheaval of unknown cause that brought an end to many other national groups, and caused multiple "peoples of the sea" to migrate elsewhere in the Mediterranean world.

An explanation for the presence of these 9th/8th century "neo-Hittites" has been avidly sought by scholars. The prevailing theory traces their origins to southeastern Anatolia, where it is assumed that a remnant of Hittite culture was preserved following the 1200 B.C. destruction of the Hittite homeland further north. It is further assumed that after a lapse of several hundred years this hypothetical group extended its influence south and east into North Syria where over time it came to dominate the Aramean peoples then living in those regions, leaving for posterity a distinctive art form and multiple inscriptions in hieroglyphic Luwian, a branch of the Hittite language preserved from the Empire days.

According to Gurney

> the language and the religion of these "Neo-Hittite" inscriptions are not those of the Hittites of Hattusas, nor are they those of the common people who had inhabited Syria under the Hittite Empire (for they were Hurrians). It seems that Syria must have been overrun by another people coming from one of the Hittite provinces, who had adopted the Hittite civilization. *The Hittites* 40

If Gurney correctly represents the situation, then reason suggests that in Anatolia itself these neo-Hittites should have been the immediate predecessors of the Phrygians, ruled by the legendary Midas, who occupied the ancient Hittite homeland by the middle of the 8th century. But the archaeological record seems to indicate that the Phrygians followed on the heels of the Empire Hittites, not the neo-Hittites. The hypothetical neo-Hittites are conspicuous by their absence throughout Anatolia, where they should in theory be most prominent. So closely do the Phrygians follow the Hittites at multiple sites, that at least in one instance the cultural remains of the Hittites and the Phrygians are intermingled through multiple layers, a situation quite impossible if the two peoples are separated in time by 450 years. According to Peter James in his *Centuries of Darkness* chapter on the Hittites:

> Paradoxically, remains of both cultures, supposedly separated in time by several centuries, have actually been found together at one site. This is Gordion, a Hittite settlement before it became the seat of the Midas dynasty. The final publication of the excavations is still awaited, but preliminary reports, together with extensive published analyses of the pottery, tell an intriguing story.
>
> Gordion is generally agreed to have been sacked by the Cimmerians in the early 7th century B.C. From before the destruction three phases of painted Phrygian ware were discerned by the excavators, representing 100 years or so, and covering the period of the great Phrygian kingdom. Soundings taken from the underlying strata of earlier phases produced completely unsuspected results. Most Hittite settlements are sealed with a clear destruction level, separating them from any traces of subsequent occupation. No archaeological relationship between the Hittites and Phrygians had therefore been envisaged. At Gordion, however, there was no such break. Instead, the two cultures appear to have co-existed for a considerable time (*Centuries of Darkness* 139-140).

Even if the Hittite and Phrygian layers had been distinct at Gordion, as they are at other locations in central Anatolia,[30] we wonder why the site would have been left uninhabited for four to five hundred years, pending the arrival of the Phrygians in the 8th century. The fall of the Hittite Empire in 1200 B.C. supposedly took place as part of a mass migration of homeless peoples, displaced by some natural catastrophe. Why was the Gordion site left vacant for upwards of 400 years? Why did some neo-Hittite group not move northward, not only to the site of Gordion, but to multiple other Hittite sites which are also assumed to have lain vacant, to fill the vast geographical void created by the demise of the Hittites. The question is all the more pertinent considering the cultural affinity and assumed historical connection between the two groups. The theory makes no sense whatever.

Absolutely no archaeological record of the existence of the hypothetical neo-Hittite peoples exists anywhere in central Anatolia where it is most expected. And this is only the beginning of the problem. In the north Syrian homeland of the neo-Hittites the reverse situation holds. It is the Hittites that are missing from the archaeological record, at least in the time frame where the traditional history would place them. The matter requires a closer examination.

The Neo-Hittites of North Syria

According to Gurney the neo-Hittite cities, mentioned frequently in Assyrian records of the 9th/8th centuries, were in most instances founded only after the demise of the Hittite empire at the end of the 13th century. Thus scholars explain the absence of Hittite artifacts at multiple sites. Only a few cities are cited as exceptions.

> The only common factors between the two epochs are Carchemish and the three cities of the Tyanitis (Hittite Tuwanuwa, Tunna, and Hupisna). Aleppo, one of the key-positions of imperial Hatti, appears as Halman and is of less importance

[30]At Boghazkeuy, ancient Hattusas, the Hittite capital, though a distinct separation exists between the Hittites and their 9th/8th century Phrygian successors, the lack of sedimentation at the separation level has led authorities to surmise that the hiatus between the two inhabitants at the site was very brief.

> than the upstart Arpad, its near neighbour to the north. All the other names are new, and many of them were probably new foundations. *The Hittites* 41

Gurney's argument is flawed. Many of the cities mentioned in the Assyrian records are indeed not named in the Hittite annals, but that does not mean they did not exist or were not part of the extended Hittite empire at various stages of its existence. The Hittite annals are primarily concerned with activities in central, northern and western Anatolia and to a lesser extent with Ugarit and the cities of Amurru on the northern Mediterranean coast. Very little attention is given to the situation in the east and south-east - Mitanni, Assyria, or north Syria. In some instances, no doubt, the Assyrians do refer to identical locations mentioned in Hittite documents but by variant names. Regardless, we single out for attention one important instance where continuity is known to exist between the two epochs - Carchemish. For now this one example illustrating the problem must be deemed sufficient.

Carchemish

It is well known that Carchemish was conquered (or reconquered) by Suppiluliumas sometime during his reign[31], and that one of his sons named Piyasili (Sarri-Kusuh in Hurrian) was installed as king of the city. The Hittites continued to hold Carchemish, and maintain their domination over its assumed Hurrian population, through the balance of the Empire period, for upwards of 150 years. Thereafter the city apparently returned to its roots until (by degrees over several centuries) it was overrun by the neo-Hittites. By the time of the Assyrian kings Ashurnasirpal II (883-859) and Shalmaneser III (858-824) the city is clearly identified as neo-Hittite.

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[31]In the traditional history it is considered that this event occurred very late, since it was during the assault on the city that Suppiluliumas received a request from the widow of Tutankhamon to send her one of his sons as a husband. And since for other reasons the death of Suppiluliumas is placed in 1335 B.C., only four years after the death of Tutankhamon, the attack on Carchemish must have taken place in a very narrow time frame at the end of the reign of the Hittite king. But even in the conventional chronology the dates of the Hittite and Egyptian kings are not firmly established, calling this interpretation into question. And the identity of the widowed queen is by no means an established fact. In the revised chronology, assuming that we are dealing with the widow of Tutankhamon, the siege of Carchemish must be dated very early in the reign of Suppiluliumas, since the death of Tutankhamon took place either before or very soon after Suppiluliumas began his kingship.

If that history is correct, the archaeological record at the site should reveal three separate strata (or groups of strata) - a neo-Hittite layer overlying a Hurrian substratum overlying the remnants of a Hittite city. Minimally there should exist clear evidence of two cities, representing the Hittite and neo-Hittite phases of occupation.

On the other hand, if the revised history is correct, and the neo-Hittite confederacies are merely vassal states on the outer fringes of the Hittite Empire, as they appear to be from the language of the Assyrian documents, we expect to find a neo-Hittite city with supposed bronze age artifacts embedded, overlying a layer devoid of any associations with the classical Hittites.

Excavation at Carchemish commenced in earnest in the early decades of the 20th century under Hogarth, assisted by Leonard Woolley et al. The work was all but complete by the beginning of WW11. The archaeologists concentrated on the larger monumental works (palace, city gates, etc.) and only marginally on the city proper. To their surprise they found that the entire corpus of monumental stone sculptures and reliefs remaining at the site, including the "Herald's Wall" and the "Water Gate", was dateable to the neo-Hittite era, showing clear signs of Assyrian influence. The pottery on the site bore unmistakable affinities with that found in the nearby Yunus cemetery, dateable to the 8th/7th century B.C. No trace of the Hittite city was found, even though the excavation in the city proper penetrated the late bronze age level. Either the Hittite city had been leveled and rebuilt, or it had not existed at all. According to Hogarth there existed no construction, no artwork, no inscription at Carchemish which could be dated before the 12th century B.C., and even that early date has been seriously questioned by subsequent generations of Hittite specialists and art-historians, who would argue that the 9th century is a more likely *terminus post quem*.

We are therefore presented with two distinct archaeological anomalies. In Anatolia proper the problem centers on the fact that the Hittite stratum is immediately followed by the Phrygian, with no indication of neo-Hittite occupation. On the other hand in Carchemish, representative of what is found at Malatya and Aleppo and elsewhere, we are confronted with a neo-Hittite layer dateable to the

9th/8th centuries with no apparent Hittite substratum. The only possible solution to both problems is to identify the Hittite and neo-Hittite peoples as contemporaries, precisely the view espoused by (in fact demanded by) the revised chronology.

At Carchemish, this thesis is supported by one additional series of finds by the excavators of the city. Though there was no trace of a Hittite city below the neo-Hittite remains, there was evidence that the Hittites and neo-Hittites *were* contemporaries. We let Peter James summarize some of the evidence from his chapter on "Redating the Hittite Empire". According to James

> many artefacts of much earlier date *were* discovered in the Neo-Hittite city. Near the Water Gate was found a stone mace-head bearing a Pharaoh's name, probably Ramesses II. On a pavement which he dated no earlier than the 9th century BC, Woolley identified 'several late Mycenaean sherds and a piece of Cypriot Iron Age ware . . . ***These must have come from the Temple Treasury where they had been preserved for many generations***. Excavating the Temple of the Storm God, he discovered a basalt stela which, as well as mentioning a 'Great King', is surmounted by a winged disc, the symbol of imperial power - it was ***evidently a relic from the Empire*** ... In a tomb securely dated to the 7th century BC, Woolley found a series of small gold figures which bear a striking resemblance to the pantheon of the frieze at Yazilikaya, conventionally dated to the 13th century BC. Hans Guterbock noted that this discovery 'links the Late Hittite period with the time of the Empire ... There is no doubt that both in style and in subjects these figures ... are Hittite in the sense of the Hittite Empire at Boghazkoy.' Yet he wondered: 'How did carvings of the thirteenth century get into a tomb of the seventh? ***Woolley himself considered that the jewellery was manufactured during Neo-Hittite times, but in a style which had, somehow, been preserved for 500 years. Guterbock preferred to see them as heirlooms, brought to Carchemish by the Imperial Hittites and 'kept in the treasury in spite of the change in domination', or, alternatively, that they had been carried there by migrating Hieroglyphic Hittites who had joined in the looting of the Late Bronze Age centres when they were sacked by barbarian invaders around 1200 BC.*** CD 128-129 (italics and emphasis added)

In quoting James we have italicized the multiple attempts by scholars to rationalize these bits of evidence from Carchemish. If nothing else they illustrate the difficulty of using the archaeological record to convincingly argue a particular thesis. Some ad hoc explanation can always be produced to explain away the most compelling evidence to the contrary.

Let the reader judge the merits of the case thus far presented. To extend our argument further we turn our attention to the Hittite Empire kings themselves, in hopes of fixing their reigns more firmly in the 9th/8th centuries where our revised history has positioned them

Suppiluliumas I and II

Only two kings by the name Suppiluliumas are known to have ruled in the entire 550 year span of Hittite history which, according to the traditional history, lasted from around 1750 B.C. through to the end of the 13th century. Scholars are generally agreed these two kings began and ended the "Empire Period" of the nation, and have assigned them the dates 1375-1335 B.C. and ca 1200 B.C. respectively. In the revised history they have been dated to the years 908-858 B.C. and 765-760 B.C. We wonder if their existence was noted by the inhabitants of Anatolia and north Syria in the 9th and 8th centuries.

Suppiluliumas I

The beginning of his reign. It is widely known that Suppiluliumas I corresponded with an Egyptian king of the late 18th dynasty. At least one Amarna letter (EA 41) purports to originate from him, addressed to an Egyptian king named Huriya, generally understood to be the Hittite rendering of kheperure, one element in the prenomen of all the terminal kings of the 18th dynasty from Akhenaten through Horemheb. If so the name could, in theory, belong to any of these kings. The recipient, if our chronology is correct, could be Ay, Tutankhamon, Smenkhkare or Horemheb.[32] Whoever he was, the letter informs us that Suppiluliumas had earlier communicated with his father and that his request at the time, presumably for gold, had been granted. We assume therefore that the

[32]Some would eliminate Tutankhamon from the list since the letter extends greeting to the king's wives and sons, but even the boy-king had at least one wife and had already fathered a son who apparently died at birth or soon after. The traditional history would eliminate Horemheb from the list of candidates since they date the Amarna letters in the period from Amenhotep III to Tutankhamon. But as stated earlier, we believe Horemheb may have participated in the Amarna correspondence.

recipient of the letter has just come into power and that Suppiluliumas was at least several years into his kingship. The present letter is a modest request for a few gold and silver statues, plus a piece of lapiz lazuli. The messenger who brought the letter was also the bearer of a number of inconsequential greeting gifts for the Egyptian king.

Because of the confused conditions of the times we are unable to definitively name the Egyptian king in question. It may have been Tutankhamon, though we suspect it was Horemheb. *Huriya* is more likely an abbreviated form of *Hera*(-em-heb) than an attempt to approximate the sound of the Egyptian "kheperure". Elsewhere we have conjectured that Horemheb's name was shortened to Zerah by Jewish historians. It is conceivable that Hera, Zerah, and Huriya are all approximations of the sound of the name of the Egyptian god Hera. Though scholars have insisted that the Amarna letters all date from the reigns of the three kings Amenhotep III, Akhenaten and Tutankhamon, we repeat our earlier suggestion that the Amarna period may have lasted briefly into the reign of Horemheb. It can be argued minimally that Horemheb used Akhetaten/Amarna as a base for governmental correspondence. In our estimation the destruction of the site awaited the reigns of Seti I and his son Ramses II, an opinion already expressed and one that is not without supporting argument.

Though we may never know precisely to whom Suppiluliumas addressed the Amarna letter EA41 we *can* estimate approximately when it was written. We have previously conjectured that Ay, Tutankhamon and Horemheb may all have come to power during the last fifteen years of the tenth century. If we are correct in our theory that Tutankhamon followed Ay, and that he in turn was followed by Horemheb, with or without an interregnum, then Tutankhamon and Horemheb began their kingships somewhere in the time frame 910-897 B.C.. We date the letter of Suppiluliumas somewhere in this range of dates (though we should point out that nothing precludes our lengthening his reign to sixty years and maintaining the chronology established early in the last chapter).

Though the traditional history suggests that the Suppiluliumas letter was written in the last years of this king's forty to fifty year rule, we clearly believe otherwise. It is hard to imagine that the writer of this document,

begging for gold, is a seventy or eighty year old man at the pinnacle of his illustrious reign, head of an empire which includes almost all of ancient Anatolia, extending beyond the Euphrates into the former domains of Mitanni, and including most of northern Syria. If anything, the letter tells us that the reign of Suppiluliumas has just begun. Ten to twenty years in the future he will establish his empire, capture Washuganni and Carchemish and extend his suzerainty over much of north Syria and the Mediterranean coast. In our earlier table of Hittite kings we dated the beginning of the reign of Suppiluliumas to the year 908 B.C., admittedly only an educated guess. The Amarna letter must date several years later.

Fortunately we can be more precise regarding the end of his kingship.

The end of his reign. It is well known that the Assyrian king Shalmanezer III, in the first year of his kingship, thus in 858 B.C., determined to extend his inherited empire into North Syria. His annals give abundant detail of his trans-Euphrates campaign, which began, while he was still east of the great River, with multiple attacks on the cities governed by Ahuni, the "king" of the state of Bit-Adini, a land mass which extended from the Euphrates to the Khabur River. He then crossed the Euphrates and fought a battle against a coalition of states including Ahuni's trans-Euphrates domains and against "neo-Hittite" forces from Carchemish and Hattina, ruled respectively by Sangara and Sapalulme. Successful in this battle he then swept across the Syrian highlands to the Mediterranean, boasting how he washed his weapons in the Sea. The inscription which records his victorious march begins

> I am) Shalmanezer, the legitimate king, the king of thej world, the king without rival, ... the son of Ashurnasirpal ... (grand) son of Tukulti-Ninurta ... a conqueror from the Upper Sea to the Lower Sea (to wit) the countries Hatti, Luhuti, Adri, Lebanon (Lab-na-na), Que, Tabali, Militene (Me-li-di)d; who has visited the sources of (both) the Tigris and the Euphrates. ANET 276

We should note that Hatti is here not a reference to some small geographical region north of Carchemish, as some scholars seem to suggest based on references to the place name Hattina used elsewhere in the inscription. The most natural reading of the text insists that we view it

as an umbrella term for the entire region of north Syria. This is clearly the meaning later in the inscription when Shalmanezer boasts

> I swept over Hatti, in its full extent, (making it look) like ruin-hills (left) by the flood .... (thus) I spread the terror-inspiring glare of my rule over Hatti. ANET 277

The Hatti land thus referenced can only refer to the Hittite possessions in North Syria, part of the extended empire of Suppiluliumas. The fact that Sapalulme appears to be associated more directly with the term Hattina (perhaps a denotation of the eastern portion of his empire) in his initial battle with Shalmanezer, is not difficult to understand. This is where one branch of the Hittite army was likely stationed under his leadership in view of the Assyrian threat. Some troops may have been garrisoned in Carchemish. The Syrian states were ruled by their own regional kings, while remaining vassal states of the Hittite Empire. Collectively these kings were referred to by Shalmanezer as "kings of the *land of the Hittites*" (^{mat}Hatti). Everything that Shalmanezer records in his annals concerning them is consistent with our impression that we are dealing here with the Hittite Empire in its initial phase. The Empire period is well underway in the first half of the 9th century B.C.!

Confirmation that we are correct, and that Sapalulme is not a regional king from an area north of Carchemish is found elsewhere in Shalmanezer's first year inscription. En route to the Mediterranean the Assyrian king crossed the Orontes and again encountered Sapalulme, who was now defending the fortress town of Alimush and is identified as its suzerain. There Sapalulme summoned aid from his dependencies, including assistance from Ahuni, chief of the land of Bit-Adini east of the Euphrates, the same man who had participated earlier in the defense of Carchemish. The domains of Sapalulme clearly extend from Carchemish on the Euphrates to Alimush by the Mediterranean.

> From the mountain of Amanus I departed, crossed the Orontes river (A-ra-an-tu) and approached Alimush, the fortress town of Sapalulme from Hattina. To save his life, Sapalulme from Hattina [called for] Ahuni, man of Adini, Sangara from Carchemish, Halanu from Sam'al, Kate from Que, Pihirim from Hilukka, Bur-Anate from Iasbuq, Ada [...] ... Assyria ...[their/his army] I scattered, I stormed and conquered the town ... I carried away as booty ..., his horses, broken to the yoke. I slew with the sword ... During this battle I personally captured Bur-Anate

> from [Izbuk]. I con[quered] the great cities (mahazu) of Hattina, I overthrew the ... of the Upper [Sea] of Amurru and of the Western Sea (so that they became like ruin-hills (left by) the flood. ANET 278

Sapalulme (Suppiluliumas) is not mentioned again in Shalmanezer's extensive annals. When next we read those inscriptions, describing visits to the Euphrates region in Shalmanezer's second and third years, the Hittite king is ancient history. We assume he died later in 858 B.C., or perhaps early the next year. If so, then he ruled Hatti for close to fifty years (908-858 B.C.). Before moving on to discuss his namesake, who ruled a century later, we make a few observations for the record regarding the first Suppiluliumas.

1) In Shalmanezer's annals we note the prominence of "Sangara from Carchemish", apparently a close ally of Sapalulme. It is assumed by scholars that this individual is the "king" of Carchemish, though in the reign of Ashurnasirpal, Shalmanezer's predecessor, this same individual was identified as a "king of Hatti". Shalmanezer fails to specify his titles. We know from Hittite documents that a brother of Suppiluliuma named Piyasili (in Hurrian - Sarre Kusuh), was appointed "king of Carchemish", perhaps as early as 885 B.C., the date when we assume the Hittites captured Carchemish, and from that base he acted as the major domo of North Syria, in command of the army. His "reign" extended at least to the ninth year of Mursilis II (849 B.C.), when he died. Sangara disappears from the scene around the same time. It is possible, though by no means certain, that Sangara and Sarre Kusuh are one and the same person. In a later section we will have cause to discuss this situation.

2) It is probable that the plague which ravaged the Hittite homeland toward the end of the reign of Suppiluliumas both precipitated Shalmanezer's tour of conquest and explains its success. Suppiluliuma might already be ill. Sickness has no doubt depleted the strength of the army.

3) By Shalmanezer's third year (856 B.C.) Suppiluliumas has died and his son Arnuwandas has suffered the identical fate. The reign of Mursilis has begun. We note from Mursilis' annals that in his first year he was beset with revolt on all fronts of his empire. He confirmed the appointment of Sarre Kusuh in Carchemish, but concentrated his effort on recovering the

province of Arzawa in south-western Anatolia. Meanwhile Shalmanezer continued to visit the Euphrates region in his second, third and fourth years, 857-855, concentrating his energy on defeating Ahuni, ruler of Bit-Adini, whose extensive domains east of the Euphrates River we have mentioned previously. By 855 Ahuni is captured and his land annexed and re-populated by Assyrians. Small wonder that Mursilis is concerned to reconfirm Sarre Kusuh and that in his annals he expresses anxiety concerning the "man of Assyria".

4) While there is no mention of Mursilis in Shalmanezer's annals, there is mention in year one of a "Mutalli from Gurgame", a region in the vicinity of Tubal, about a third of the way from Carchemish toward Hattusas, the Hittite capital. Apparently some of Shalmanezer's troops journeyed slightly north of the direct route from Carchemish to the Sea, and received "the tribute of Mutalli from Gurgume (to wit): silver, gold, large and small cattle, wine (and) his daughter with her big dowry" (ANET 277) If Mursilis was in his forties when he became king, his son Muwatallis could easily be the king in question, delegated the responsibility of guarding the route to the Hittite homeland. The resemblance of the name is striking. We have already argued that by Mursilis' fourteenth or fifteenth year Muwatallis was likely elevated to kingship to assist his father.

5) The critic might argue against our thesis by noting that Shalmanezer's father Ashurnasirpal (883-859 B.C.) refers to a "city of Kunulua, the royal residence of Lubarna from Hattina". If Suppiluliuma ruled from 908-858 B.C. (or thereabouts), it might fairly be asked why a king Lubarna was ruling in that same time frame. There are two possible answers to the objection. In the first place the text does not state, nor provide any hint, that this Hittite king was the "great king of Hatti" as opposed to a relative of the king ruling over the specified city. Labarna was the name of the first great king of Hatti, borne by at least one other "great king", Hattusilis I, who used it as an alternative name, this long before the Empire period. Who knows how many Hittite princes bore the same name in generations following. On the other hand, the name may refer to Suppiluliumas himself, possibly as an alternative name, but more likely as a title. It is well known that the title par excellence of the Hittite kings was Tabarna (meaning "great king") and that this name probably derives from the

name of Labarna, the revered first great king of Hatti. It is assumed by scholars that the original Hittite name had a sound intermediate between Tabarna and Labarna, leading to confusion as to how to pronounce it. The initial letter varies in the inscriptions. We assume therefore that the Assyrians merely interpreted the title as a personal name, and that Labarna in the annals is nothing more than the title "great king", used in reference to Suppiluliumas I[33].

Having said that, we move on to the second of the namesake kings.

Suppiluliumas II

In the traditional history Suppiluliumas II ruled for an unspecified number of years around 1200 B.C.. He was probably, though not certainly, the son of Tudhaliyas IV and he apparently replaced his brother Arnuwandas IV on the Hittite throne when this king died. Thus the second of the namesake kings ruled about 135 years after the death of Suppiluliumas I (1375-1335 B.C.). In the revised history we have shortened the time span from the death of the first to the beginning of the reign of the second Suppiluliumas to approximately 90 years, most of the change resulting from our shortening of the reigns of Mursilis II and Muwatallis. We have dated the reign of Suppiluliumas II to the years 765-760 B.C. We could be in error by as much as three or four years. Regardless, his reign must lie in the middle decades of the 8th century. We should seek for information regarding him from those living around that time. Fortunately, record of one of his military campaigns has recently come to light.

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[33]We know that the annalists of Shalmanezer once made reference to a king Mari of Damascus, mistaking the Semitic honorific title mari ("my lord") for the name of the king. Here Labarna (or Tabarna) may be mistaken for (if not actually) a legitimate alternative name for Suppiluliuma. We quote from Gurney in defense of our suggestion. "The kings of the Old Kingdom style themselves 'Great King, tabarna'. The title 'Great King' belongs to the language of diplomacy and denotes the Hittite king's claim to be one of the great powers of the time, with dominion over lesser kings. Tabarna is probably nothing but the name of the ancient forebear Labarnas in a disguised form. The title is borne only by living monarchs, and it is thought that each reigning king was regarded by the Hittites as the incarnation of the founder of the royal line. The variation in the initial letter would indicate that the original (Hattian?) form of the name contained a peculiar consonant which the Indo-European Hittites were unable to pronounce." Gurney, *The Hittites*, p. 64.

The Incirli Stele. In 1993, during an archaeological survey in the Karamanmarash region of southeastern Turkey, in the village of Incirli, Elizabeth Carter, working for the UCLA archaeological team, discovered an iron age stela very badly preserved.[34] Using sophisticated imaging techniques a preliminary transliteration and translation was produced several years later, with work still ongoing. A description of the stela and tentative description of its contents have been published.by the translators Bruce Zuckerman and Stephen Kaufman.[35]

> The Incirli Stela contains a lengthy text written on all four sides of the stone in standard Phoenician of the late 8th century BCE. It is a commemorative boundary inscription marking the successful end of a territorial struggle between the kings of Cilicia (Que) and Kummuh and the various allied powers, presumably over the territory where the monument was originally erected. Since it seems clear that the monument was reused much later as a boundary stone with a Greek inscription of the Byzantine period, we cannot necessarily assume that the earlier text should be associated with the specific locale where the stone was discovered in 1993. Still, considering its size and weight, it seems unlikely that it had been moved very far from where it was first erected.
>
> In the first part of the inscription, the subject whose exploits are commemorated in the first person narrative (apparently, King Awarikku of Que, known previously from the famous Karatepe bilingual inscription) recounts two successive battles - an earlier battle serving essentially as a prelude to the conflict that is the main concern of the text. ***This earlier battle was instigated by Suppiluliumas of Kummuh against Que and the Danunites.*** The inscription provides totally new information about the Luwian city states of this period. Apparently, Que was originally a part of a larger kingdom (of the Danunites) centered at Tabal, for the last known king of Tabal - Wasurmas - was the father of the writer of the Incirli Stele and is described here as king of the Danunites. Tabal was conquered by the infamous Warpalawas (ally of Midas), leaving Que itself as the sole domain of the Danunites. It appears that the first war described in our text involves a recounting of this setback for the Danunites. According to our text, the king commemorated on this stele then killed Warpalawas, prompting the subsequent war described in the second half of the text. (italics & emphasis added)

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[34]See E. Carter, "Report on the the Kahramanmarash Archaeological Survey Projedct from 24/9/93 to 11/11/93." Arastirma Sanuclari Toplantisi 12 (1995) 331-341 and "The Kahramanmarash Archaeological Survey Project: A Preliminary Report on the 1994 Season." Arastirma Sanuclari Toplantisi 13 (1996) 289-306.

[35]The only publication known to the author is the UCLA website dedicated to the zinjirli find. The website http://www.humnet.ucla.edu/humnet/nelc/stelasite/zuck.html contains the Zuckerman & Kaufman article.

We make several observations regarding the Incirli stele as interpreted by the two named scholars.

1) The inscription is most reasonably dated to late in the third quarter of the 8th century, thus around the year 735 B.C., based on several criteria. In the first place the translators have determined that the inscription is composed in the "standard Phoenician of the late 8th century", which could suggest a date as late as 700 B.C. Secondly, the king Awarikku, son of Wasurmas, who authored the Incirli stele, is known from the Karatepe inscriptions, which scholars have dated as early as the late 9th century to as late as the second half of the 8th century The two factors together suggest a date earlier rather than later in the second half of the 8th century.

2) Wasurmas, the father of Awarikku (and thus a generation removed from the stele's author) was apparently the last king of the Danunites at a time when their kingdom extended northward into Tabal. Involved in the downfall of the Tabal area of the kingdom was "the infamous Warpalawas" an ally of a Phrygian king named Midas. This cannot be the "Midas (Mi-ta-a) king of Musku" repelled by the Assyrian king Sargon (722-705 B.C.) during an invasion of the Kasku lands and the regions of Tabal and Cilicia sometime early in his reign.[36] It is claimed by scholars that this later Midas ruled approximately from 728-688 B.C. when he died in an invasion by the Cimmerians. This would necessitate dating Awarikku (and thus the Karatepe inscriptions which name him) in the middle of the 7th century, where they clearly do not belong. Besides, the Incirli stele knows nothing of an invasion of Tabal by Sargon; suggesting instead that Warpalawas (and Midas? and Suppiluliumas?) were the invaders. The Midas of the Incirli inscription must be two generations removed from the namesake who ruled in the time of Sargon. He was probably the grandfather of the latter. If we are correct regarding this earlier Midas we should date him around the years 770-750 B.C.

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[36]ANET 284 The relevant text is from an undated inscription in which Sargon describes himself as the one "who exterminated Kasku, all Tabali and Cilicia (Hilakku), who chased away Midas (Mi-ta-a) king of Musku, who defeated Musur (Mu-su-ri) in Rapihu, etc. Elsewhere in an inscription dated to his fifth year (ANET 284) he notes that "Pisiri of Carchemish broke the oath sworn by the great gods and wrote messages to Midas (Mi-ta-a), king of Muski, (full) of hostile plans against Assyria" This later Midas may well be the legendary king whose touch turned profane objects into gold, or he may be his grandson. There is some evidence that the Phrygian kings used the names Midas and Gordion alternatively through much of their history.

3) Confirmation of our opinion concerning Midas can be found in the Hittite archives. In the correspondence belonging to the reign of Arnuwandas IV, the immediate predessesor of Suppiluliumas II, there is record of the activities of a king Mitas (Midas) in north-western Anatolia. We listen to Gurney describe the situation.

> But the days of the Hittite Empire were already numbered. Under the next king, Arnuwandas IV, the situation in the west rapidly deteriorated. Madduwattas made common cause with Attarissiyas, and though the Hittite king in a lengthy rescript addresses him as nothing more than a disloyal vassal, we sense an entirely new situation in that area. In particular, we are told that Madduwattas took the whole land of Arzawa. "At the same time another adventurer names Mitas was active in the eastern hills where had formerly been the kingdom of Hayasa; the identity of his name with that of the king of the Mushki of the eighth century B.C. who is usually equated with the Phrygian Midas of Greek tradition, may be no more than *coincidence*, but it is possible that the Mushki (classical Moschi) were already in this region and that Mitas was a dynastic name. *The Hittites* 38-39 (italics added)

Coincidence indeed. Arnuwandas and Mitas, according to the history followed by Gurney, lived at the end of the 13th century. Midas, the Phrygian king driven off by Sargon, lived at the end of the 8th century. The persistence of the name Midas in the identical region of Anatolia for over 500 years is highly unlikely. Besides, evidence is lacking that the Phrygians had even arrived in Anatolia before the 9th century.

With Arnuwandas moved to the years 775-765 B.C. the coincidence disappears entirely, yet another by-product of the errant Egyptian chronology which concerns us in this series. The end of the reign of Arnuwandas lies approximately where we have positioned Wasurmas, the father of Awarikku. It must therefore be the time of the king Midas referred to in the Injirli stele and of the Suppiluliumas who conducted one of the wars which ended the Tubal kingdom of the Danunans. And since, in the revised history, the Hittite king Suppiluliumas II followed Arnuwandas II, and ruled for several years, there should be no doubt that the neo-Hittite and Hittite kings by this name are in fact the same person.

We let Gurney continue his discourse, describing the death of Arnuwandas and the end of the Hittite Empire.

> Be that as it may, we know that great mass-movements of population were afoot which the brittle Hittite confederacy was utterly unable to withstand. The edicts of Arnuwandas contain no hint of the approaching doom, and he may not have lived to see the catastrophe. A single document suggests that he was succeeded by his brother, a second Suppiluliumas, but the reign of the latter must have been short. The records of Ramesses III tell how the isles were disturbed and the Hittites with other peoples fled into Syria in a great invasion which, in conjunction with the 'Peoples of the Sea', menaced Egypt ... In Asia Minor, to judge from Homeric legend, the Phrygians soon replaced the Hittites as the dominant power. *The Hittites* 39

It is said by scholars that only two Hittite kings by the name of Suppiluliumas ever lived. They were separated in time by over a century. These same scholars have suggested that the Sapalulme who fought with Shalmanezer III was only a regional neo-Hittite king, though no trace of his kingdom has ever been found and no artifact belonging to him has ever been discovered. It remains to be seen what explanation will be forthcoming for the second neo-Hittite Suppiluliumas, whose reign a century after his namesake is even more problematic. The region of Kummuh from which he launched his attack on the Danunans lies a short distance north-east of the Gurgum region, fully within the territory of the ancient Hittites. It is in the Gurgum region where Mutalli (Muwatallis?) ruled in the days of Shalmanezer III (see above).

It is surely more than coincidence that the reigns of these two neo-Hittite kings by the name Suppiluliumas lie in the identical time frame that we have assigned to their Hittite counterparts in the revised chronology. And how fortunate for our revision that a king Midas is conveniently present at the time of the second Suppiluliumas in both instances.

Other Parallels

Having established that Suppiluliumas I and II lived at the beginning of the 9th and middle of the 8th centuries respectively, we wonder if parallels might be found for those kings intermediate between them. It is unfortunate that we cannot use the established synchronisms between Muwatallis, Hattusilis III and Ramses II to buttress our argument, since they were used to fine tune the positioning of the Hittite Empire in its revised setting. If we are to avoid a circular argument what is needed are

links between the intermediate Hittite kings and the Assyrians. Fortunately there do exist documents which provide such cross references, though the evidence is mitigated somewhat by the fact that the Assyrian names used in the Hittite texts were used by more than one Assyrian king. For the record, however, we cite the evidence.[37]

At least one document (KBo I 20) makes explicit reference to a king Adad-nirari, though unfortunately the Hittite king is not named. It is attributed to Hattusilis III by scholars, based exclusively on the traditionally accepted dates for Adad-nirari I and Hattusilis III. We assume it was written by Tudhaliyas IV (800-775), who was contemporary with Adad-nirari III (791-782).

Fully three documents have been held to synchronize the Assyrian king Shalmaneser I with the Hittite kings Hattusilis III and Tudhaliyas IV, again based largely on the dates of these kings in the traditional history. Two of the documents in question (KUB XXIII 99; and RS 34.165) do contain the name of a king Tudhaliyas, but the name of Hattusilis is entirely absent from the third (KUB XXIII 88). We assume all three originate from Tudhaliyas IV (800-775) but that the recipient is Shalmanezer IV (781-772).

A single document (KUB XXVI) was written by an unnamed Hittite official (king?) to an Assyrian prince named Tukulti-Ninurta. In addressing Tukulti-Ninurta the sender makes reference to "the king of Assyria your father to whom Urhi-Teshub had (previously) written", a remark that establishes two things: 1) that the sender is most likely Hattusilis III or Tudhaliyas IV, the two kings who followed immediately the reign of Urhi-Teshub, and 2) that the recipient is the son of the former king, and apparently not a king in his own right. We mention the document only because scholars have used it to establish a correspondence between Tudhaliyas IV and Tukulti-Ninurta I, something it definitely does not do. In the revised history the recipient, the prince Tukulti-Ninurta, must have been a son of Shalmanezer III, assuming we

[37]For a description of the contents of the following Hittite inscriptions we rely entirely on the description provided by Peter James and his colleagues in Appendix 4 of their Centuries of Darkness. This author has unfortunately (due to illness) not had opportunity to research these documents first hand. This task accomplished, I hope to return to this section and modify a few statements.

take the term "father" literally.[38] The reign of Shalmanezer III (858-824) overlaps the reign of Urhi-Teshub (832-825) in the chronology outlined above.

One Hittite document alone could be considered a problem for the revised history. KUB III 74 is a letter sent from a king Tudhaliyas to a king Tukulti-Ninurta, absent any indication as to which Tudhaliyas wrote the letter or which country the recipient ruled over. The traditional history considers this document as support for their contention that Tudhaliyas IV was a contemporary of the late 13th century Assyrian king Tukulti-Ninurta I, a stretch considering the lack of specificity in the naming of the key players. The "Centuries of Darkness" authors consider the same document, in combination with KUB XXVI (see above), as support for their thesis that Tudhaliyas IV was a contemporary of the tenth century Assyrian king Tukulti-Ninurta II. Both groups consider that the Tukulti-Ninurta in both KUB III 74 and KUB XXVI are the same person. They may be correct, but the matter is certainly not well established.

If we assume that the two references to Tukulti-Ninurta are to the same person and that this individual is an Assyrian prince/king (an assumption we are by no means obliged to make), there are two scenarios which might account for all the data. Both assume that this dignitary was the son of Shalmanezer III.

The first possibility results from the chaotic state of affairs which prevailed at the end of the reign of Shalmanezer III. It is well known that during the final ten years of his reign this Assyrian king faced increasing opposition at home from within his own family. His ambition to extend his empire abruptly ceased. "After a last attempt to conquer Damascus in 838 B.C. the Assyrian confessed his failure by leaving Syria alone for the rest of his reign" (Roux, Ancient Iraq, p. 276). [As a result, both Hatti and Egypt renewed their interest in the area, resulting in the ill fated but famous battle of Kadesh in 835 B.C. between Hattusilis and Ramses II] In the final years of Shalmanezer's reign there erupted a full blown civil war, which extended five years beyond his death. According to the historian Georges Roux:

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[38]The assumption that Shalmanezer would name a son after his grandfather Tukulti-Ninurta II, is entirely reasonable. He boasted of his descent from this king in many of his inscriptions.

> The end of Shalmanezer's long reign was darkened by extremely serious internal disorders. One of his sons, Ashur-danin-apli, revolted and with him twenty-seven cities, including Assur, Nineveh, Erbil and Arrapha (Kirkuk). The old king, who by then hardly left his palace in Nimrud, entrusted another of his sons, Shamshi-Adad, with the task of repressing the revolt, and for four years Assyria was in the throes of civil war. The war was still raging when Shalmanezer died and Shamshi-Adad V ascended the throne (824). With the new king began a period of Assyrian stagnation which lasted nearly a century. *Ancient Iraq* 277

The Assyrian king lists and the Eponym Canon assume that the reign of Shamshi-Adad began in the year 823, immediately following the death of Shalmanezer V. But in fact it took this young king at least five years to recover much of the territory lost to his rebellious brother(s). Thus for up to ten years the cities of Assur and Nineveh (and others) were ruled by other sons of Shalmanezer V, none of whose names appear in the king lists of Assyria. We assume as a possibility, that one of these prince/kings, all sons of Shalmanezer III, was named Tukulti-Ninurta. It was a common name in Assyria.

A second possibility takes us to the end of the reign of Shamshi Adad V (823-810). When this son of Shalmanezer III died in 810 B.C. the kingship passed to his son Adad-Nirari, who was still an infant. The Assyrian eponym canon assumes that Adad-Nirari began his kingship immediately. But in fact others ruled in his stead during his infancy. There are conflicting theories as to what transpired over the first five to ten years of his childhood, until the young boy reached maturity. According to some sources Assyria was ruled by the wife of Shamshi-Adad, a queen named Shammuramat. Others claim that much of the governing of the country should be credited to an official named Bel-tartsi-ilu-ma, the dedicant of many inscriptions, whose authority extended over several provinces. Some early scholars identify Shammuramat as the wife of this official. We see no reason why under these circumstances an elderly son of Shalmanezer III named Tukulti-Ninurta, an uncle of the boy-king, might not have had a hand in governing a portion of the country. If so, we understand why he is omitted from the Assyrian king-lists. We also understand the ambivalence of the Hittite authors in addressing this dignitary.

We admit that these are purely hypothetical constructs. We have no proof that Shalmanezer had a son named Tukulti-Ninurta, nor that he

acted in a quasi-regal capacity during the civil upheal which ended his father's reign or during the infancy of his nephew. But barring evidence to the contrary these remain possibilities and they do explain the details of the Hittite correspondence. And they are just two among many hypotheticals. The matter needs to be investigated further.

We have omitted one further important set of synchronisms, those between Tudhaliya IV and the Achaeans and the Trojans of Homeric legend. They deserve a chapter unto themselves.

We have also omitted a detailed year by year comparison between the reigns of the Hittite kings, insofar as they are known, and the reigns of the Assyrian, Syrian and Israelite kings of the 9th and 8th centuries. While such an analysis might prove helpful it would detract at the moment from the broader picture we are attempting to create. Both in the comments above, and in those which immediately follow we are painting with a wide brush, taking the role of the classical underpainter. Let others with more patience and skill add detail to the canvas.

Possible Objections

We have omitted till the last a response to three anticipated objections to our identification of the Hittites and the neo-Hittites. The first relates to the kings of Carchemish and a second to the supposed achilles heel of all revisionist endeavors, the assumed synchronism between the Assyrian king Ashuruballit I and the Hittite king Supilluliumas I. The third, lest some observant reader should raise the issue, concerns the Mitanni. We begin with the Carchemish kings.

Kings and Rulers of Carchemish.

Resulting from the archaeological survey conducted at Carchemish (see above), and augmented by Hittite and Assyrian records which name some of the rulers at Carchemish, scholars have pieced together the following chronology of the "kings" of this city.

Table 9: Kings of Carchemish (Traditional History)

Name	Approximate Dates	Contemporary of …
Piyasili (Sarre-Kusuh)	1300 B.C.	Mursilis II
Sakhurunuwa [...]-Shurruma		
Ini-Teshub	1220 B.C.	Arnuwandas IV/Suppiluliumas II
Talmi-Teshub	1180 B.C.	Ramses III
Gap in the Hittite records		
Ura Tarhundas		
Suhis I	970 B.C.	
Astuwatimanzas		
Suhis II		
Katuwas	900 B.C.	
Sangara	880-848 B.C.	
Astiruwas	820 B.C.	
Kamanis	790 B.C.	
Pisiri	738-717 B.C.	Tiglath Pilezer & Sargon II

A problem clearly emerges when we lower the dates for the Hittite Empire by approximately 450 years, placing it in the time frame 900-760 B.C. The revised location of the Empire period compels us to revised downward the dates for the "kings" Piyasili, Ini-Teshub and Talmi Teshub by roughly the identical 450 years, placing them in the 9th and 8th centuries, in the same time frame occupied by previously identified rulers of the city. As a result there emerge two distinct series of "kings of Carchemish" for the time span encompassing the reigns of Mursilis II through Suppiluliuma II, and for perhaps as much as twenty years beyond, thus roughly from 856-750 B.C. This new situation is charted below in table 10.

Table 10: Hittite Governors and Native Kings
of Carchemish (Revised History)

Native Rulers	Hittite Appointees (Kings)
Ura Tarhundas	
Suhis I ca 970 B.C.	
Astuwatimanzas	
Suhis II	
Katuwas ca 900 B.C.	
Sangara (885-848?)	Piyasilis (Sarre Kusuh) 885-848?
Sakhurunuwa	
Astiruwas	Ini-Teshub
Kamanis " ca 790 B.C.	
Sasturas (vizier)	Talmi-Teshub ca 765 B.C.
	Kuzi-Teshub ca 750 B.C.
Pisiri 738-717 B.C. time of Tiglath Pilezer & Sargon	

It is assumed by some critics, in comments addressed toward other revisionist works, that this dual line of Carchemish "kings" constitutes a major problem for the revisionist. In fact, the situation is precisely what is expected from our knowledge of the political landscape during the ascendancy of the Hittite Empire, while it struggled to maintain its hold on the Hatti lands of north Syria and stave off the growing power of Assyria. Concerning our table 10 we make the following comments:

1) It goes without saying that our table differs considerably from that which arises in other revisionist endeavors, since we have lowered the dates for the Hittite Empire by a substantially larger period of time. Thus the details of our explanation differ somewhat from those provided by other authors. There remains, however, a degree of commonality.

2) When the authors of "Century of Darkness" were criticized for assuming the existence of two contemporaneous lines of kings of

Carchemish, they pointed out, quite correctly, that the archaeological record supports such a situation. According to Peter James:

> The Lion Gate sculptures find their closest parallels in those produced by the 'Suhis dynasty' at Carchemish, dated to the late 10th century B.C. Suhis and his descendants clearly commissioned and inscribed these sculptures, yet their role as rulers of Carchemish remains mysterious. The titles they gave themselves were restricted to the relatively modest tag: 'Lord of the country of Carchemish'. Hovering alongside them, a presence which Hawkins has increasingly noted during his collation of all available Neo-Hittite inscriptions, is another line of kings who seem to claim greater titles. CD 135

It is known from many Hittite historical documents that when the Hittites captured foreign states they often allowed the native rulers to continue ruling, after first establishing treaty relationships with strict sanctions. We know that Suppiluliumas first conquered Carchemish early in his reign, thus early in the 9th century, and immediately assigned his son Piyasili, whose Hurrian name was Sarri Kusuh, as "king of Carchemish". This does not imply that the native ruler of the Carchemish district was deposed. The possible name of this ruler, Sangara, is preserved in the Assyrian records of Ashurnasirpal and Shalmanezer III. There is no contradiction here. We know that Sarri Kusuh functioned primarily as the leader of the Hittite army in North Syria. Carchemish was merely his base of operations. His appointment was later confirmed by his brother Mursilis immediately after Mursilis became king. Immediately after, the army was sent to Carchemish under command of Sarri Kusuh in order to stave off an imminent threat from an unnamed Assyrian king, whom we identify as Shalmanezer III, who in 857 was quelling a revolt in the province of Bit-Adini east of the Euphrates. But Mursilis was threatened on all fronts of his empire. In his third year, 855 B.C., Sarri-Kusuh and the army were summoned back to Hatti to assist in the war on Arzawa in eastern Anatolia. Several years later he and the army were fighting to subdue revolt in Amurru on the Mediterranean coast. In Mursilis' 9th year Sarri-Kusuh died. Under no circumstances can scholars justify their assertion that this Hittite general was the ruler of the city of Carchemish, regulating its daily affairs, even if his title is suggestive of such a function. Therefore if Sangara, mentioned in the inscriptions of Ashurnasirpal and Shalmanezer III, is identified as the hereditary regional king, distinct from Sarri Kusuh, there can be no serious objection by critics. There is no contradiction, though we do

reserve judgement on that explanation. There is a possibility that Sangara is simply another of Sarri Kusuh's names. This suggestion of alternate names may also apply to the other kings.

3) While we are not surprised at the existence of a plurality of kings centered at Carchemish we do question whether the preserved names reflect this situation. We have already suggested that Sangara may have been an alternate name of Sarri-Kusuh. A similar proposal could be made regarding the three other supposed local rulers whose terms in office coincided with those of Hittite appointees. We wonder if Astiruwas, Kamanis, and/or Sasturas are simply alternative names of Ini-Teshub, Talmi-Teshub, and Kuzi-Teshub. The former epithets are perhaps their native names, the latter are clearly Hurrian. If these are Hittite appointees they may also have had Hittite names and possibly variants in the multiple language groups which existed within the Empire. And the dates assigned all of these individuals are merely approximations. Thus the respective pairs of named "kings" may have ruled at precisely the same time. If so the probability that they are one and the same person increases.

4) We notice that the line of rulers of Carchemish in the revised chronology of table 10 extends backward only to the 10th century, agreeing precisely with the archaeological record at the site, which suggests that all of the monumental remains date from the neo-Assyrian period. The traditional 12th century dates for the earliest Hittite "kings of Carchemish" stands in stark contrast to the archaeology of the city. We note, additionally, that the traditional history is compelled to assume a gap of several hundred years, artificially created, in the line of kings of Carchemish. This "dark age at Carchemish" mirrors that which occurs elsewhere in the near east. We know its source.

Ashuruballit

Without doubt the argument most frequently used by the traditional history in defense of its 14th-13th century date for the Hittites is the assumed synchronism between Ashuruballit I of Assyria, the Hittite king Suppiluliumas I, and the Amarna period of Egypt.

We earlier began our listing of the Assyrian kings with Adad Nirari II (909-889 B.C.), this for two reasons. On the one hand Adad-Nirari is about the earliest of the Assyrian kings whose dates are relatively certain, confirmed by the Assyrian Eponym Canon. On the other hand, our interest at the moment was focused on the Amarna period and later, and the Amarna letters in our estimation dated from the time of Adad-Nirari (or at most a decade of two earlier). But scholars have created an extended list of Assyrian kings going back hundreds of years from the late 10th century, based on a number of "king lists". And according to those lists a king Ashuruballit, the first of that name, ruled Assyria roughly in the time frame 1365-1330 B.C., his reign overlapping that of the Hittite king Suppiluliumas I (1375-1335 B.C.)

In the Amarna archives there exists two letters, EA15 and EA16, that supposedly originate from this king Ashuruballit, thus identifying him as a contemporary of Suppiluliumas I who also authored an Amarna letter. In the opinion of many scholars this supposed synchronism, in and of itself, confirms the placement of the Hittite Empire in the 14th/13th centuries. Clearly one of three responses to this situation must prevail: 1) the association of the Ashuruballit in the Amarna letters with the first Assyrian king bearing this name must be incorrect, or 2) the dating of Ashuruballit I in the accepted Assyrian chronology is incorrect, or 3) our revision is discredited and should be rejected.

Clearly we reject the third possibility. We have established our chronology carefully and in dependence on solid evidence. It is the "facts" in this instance which must be challenged. And while we agree with critics of the extended Assyrian king list that the list is flawed and in need of serious revision, we do not believe that the solution to the problem lies in re-dating Ashuruballit I. This leaves us with but one possibility. The Ashuruballit in the Amarna letters cannot be the Ashuruballit I of the king lists. And clearly, since we have dated the Amarna letters to the last three decades of the 10th century, this Ashuruballit must be a contemporary of either Ashur-dan II (931-909) or Adad-nirari II (909-889). If he was an Assyrian king then he must have ruled over some branch of the Assyrian empire or over a rival kingdom. The matter needs looking into.

We begin our discussion by quoting an extensive summary of the problem from Peter James and the authors of *Centuries of Darkness*, fellow revisionists faced with the same dilemna. In an appendix to their compendium they discuss the issue, discounting the identification with Ashuruballit I, and raising the possibility that the name should be identified with some otherwise unattested ruler, essentially the same solution we propose.

> The only synchronism between named kings of Egypt and Assyria during the Late Bronze Age is provided by two letters from the El-Amarna collection (EA 15, 16). These were written by Assuruballit, King of Assyria, one (EA 16) being addressed to Pharaoh Naphuria, the cuneiform version of Neferkheprure, prenomen of Akhenaten. The author of the two letters is assumed to be the Assuruballit known from the Assyrian King List and dated by its chronology to the 14th century BC. Although universally accepted, the identification is not without problems. In EA 16 Assuruballit mentions that his father Assur-nadin-ahhe corresponded with Egypt; yet the King List and the available monuments agree in describing Assuruballit as the son of Eriba-Adad. In his introduction to the inscriptions of Assuruballit I, Luckenbill reviewed a possible explanation:
>
> The word 'father' may here have the meaning 'ancestor', as often in the Assyrian texts, but even so our difficulties are not all cleared up. In the texts given below Assur-uballit does not include Assur-nadin-ahe among his ancestors, although he carries his line back six generations.
>
> While the El-Amarna letter may well reflect some other relationship (e.g. adoptive) other than direct filiation between Assuruballit I and an Assur-nadin-ahhe, this is merely hypothetical, and the possibility remains that the El-Amarna correspondent was not the Assuruballit son of Eriba-Adad known from the monuments, but another, as yet unattested ruler. Thus the much vaunted synchronism between Akhenaten and Assuruballit I, the main linch-pin between Egyptian and Assyrian Late Bronze Age chronologies, is flawed and must be treated with caution. CD 341

It is claimed by critics of this "unattested ruler" thesis that the theory is indefensible, that there exists no evidence of the existence of multiple rulers within the Assyrian empire. But that is absolutely not the case. The Assyrian kings were continually faced with revolt from within their own ranks, with defections within their own family, with challenges to their authority from officials left in charge of the southern (Babylonian) or western domains. Frequently they faced assassination attempts. While it is true that no other king of Assyria by the name Ashuruballit is known from the monuments, or from the king lists, the critic should be careful not to read too much into this evidence. There is one other Assyrian king Ashuruballit. He ruled briefly at the end of the neo-Assyrian era in the

late 7th century, and is similarly unknown from the monuments and the king lists. Were it not for a casual reference in the Babylonian Chronicles we would never have known he existed. And there is compelling evidence in the "geography" of the Euphrates region to suggest the reasonableness of the assumption that another Ashuruballit may have lived and ruled in that region of the country in the late 10th century. The argument is circuitous but needs to be presented.

The reader needs to be mindful of one fact as we proceed. The map of the near east at the time of the Amarna letters is seriously flawed. Those letters are assumed to originate from the 14th century, and the map of the Euphrates region at that time is based largely information drawn from multiple inscriptions purporting to originate from 18th dynasty kings such as Thutmose III. And we have argued strenuously in our second book that many of the key documents supposedly originating from those 18th dynasty kings actually belong to the 7th century where they shed light on the political landscape of a much later time. In the revised history we argue that the Amarna letters belong to the late 10th century, when the political situation was markedly different than is assumed for a 14th century context. Much of ancient near eastern history will have to be rewritten and the accompanying geography drastically altered, once it is accepted that the 18th dynasty and the Amarna letters belong to the 10th century, and that the peoples and events represented therein followed on the heels of the Aramean migration into the Euphrates region and Syria. This migration supposedly began in the 11th century or earlier, completely revamping the political landscape.

When we examine the political situation that actually existed in the Euphrates region in the 10th century B.C. we see a possible answer to two questions of interest to us. “Why, if the Amarna letters originate from the time of Ashur-dan II and Adad-nirari II, did neither of these two individuals author a letter in the Amarna archives?” And “From what region within Assyria did a ruler named Ashuruballit write two letters to an Amarna king?

Aramean States

We encounter several obstacles when we seek to establish the geo-political landscape at the bend of the Euphrates during the 10th century B.C. It is known that kings by the name Ashur-resh-ishi II (ca. 970) and Tiglath-pilezer II (ca 965-935) ruled Assyria prior to Ashur-dan II (931-909) and Adad-nirari II (909-889). Unfortunately any relevant inscriptions authored by the first three kings in this sequence have not survived. We are first informed of the state of affairs in the western provinces in a brief inscription of Adad-nirari. The situation improves considerably during the reigns of his successors Ashurnasirpal (889-858) and Shalmanezer III (858-824) where multiple lengthy inscriptions bring us into the full light of an Aramean world, with Aramean cities and Aramean peoples in full control, speaking their own language, even if from time to time they are compelled to pay tribute to Hittite or Assyrian overlords.

From the inscription of Adad-nirari, informed by those of his successors, we can deduce the situation that prevailed under the three kings who preceded him. Hugo Winckler, writing at the beginning of the 20th century, summed it up briefly:

> Each of these three kings bore the titles "king of the world, king of Ashur," which henceforth were constantly assumed. *Harran* and *Ashur* are the chief cities of the two parts of the land. But the one part is held entirely by an Aramean population who in the old cities caused the old population the same troubles that the Chaldeans prepared for the Babylonians, and it contained besides a number of Aramean cities whose princes seized every opportunity to strike for independence or *even the reins of government*. Near to Harran there stood an Aramean state, Bit-Adini, a counterpart to the dukedom Edessa during the Crusades, just as the Chaldean Bit-Dakuri existed near Babylon. Others still we shall have to note in the time of Ashur-natsir-pal.[39]

By the time of Shalmanezer III there were multiple Armaean states lying between Assyria and the Euphrates. Both Shalmanezer and his father Ashurnasirpal spent considerable time attempting to recover these lands for Assyria. On the assumption that they were once part of the Assyrian

[39]Hugo Winckler, *The History of Babylonia and Assyria* (1911), p. 210.

Empire we do not know precisely how or when they were lost. The 10th century history of the region is essentially lost to us.

Of these Aramean states one in particular stands out from the others. When we documented the encounter between Sapalulme (Suppiluliumas) and Shalmanezer III in the year 858, we noted that Suppiluliumas was allied with Ahuni, "king" of Bit-Adini, a massive region extending from the Euphrates to the Habur east of the Euphrates, with added domains west of the great River. It may or may not have included the city of Harran. The historians Hallo and Simpson explain:

> This area, lying between the river Balih and the westernmost part of the Euphrates, was ruled by the Arameo-Hittite Ahuni from his fortress at Til Barsib (modern Tell-Ahmar) on the east bank of the Euphrates. In three successive campaigns (857-855), Shalmanezer chased Ahuni from his capital and renamed it after himself, annexed Bit Adini to Assyria, and captured Ahuni.[40]

This is the same state, with capital at Harran, that apparently dominated these territories in the 10th century and later into the reign of Ashurnasirpal. We know that Ahuni's reign extended back into the reign of Ashurnasirpal (883-859 B.C.), but the rulers of this extensive land mass in the days of Ashur-dan II (931-909) and Adad-nirari II (909-889) are not known. We suspect, though we cannot prove, that at the time of the Amarna letters, the "governor" of the western region later known as Bit-Adini was named Ashuruballit, the state not yet having fallen into Aramean hands. If, as suggested by Winckler, the Aramean rulers who later dominated the region aspired to "the reigns of government", we can readily understand why the Assyrian governors of the region who preceded them might have had the same aspirations. As we explain below, they would likely have been identified as "kings of Assyria" in correspondence with foreign dignitaries.

Though we have suggested the possibility, it is not necessary that we credit Ashuruballit with political ambitions or view his state as a renegade from the Assyrian empire. That would be reading too much into his use of the title "king of Assyria" in a letter to an Egyptian pharaoh. The employment of such titles was rather reckless in the near eastern world of

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[40]William Hallo & William Simpson, The Ancient Near East: A History (1971) 127

this time. The situation in Egypt is a case in point. We repeatedly noted in the first book of our series that many times multiple kings ruled simultaneously in various regions of ancient Egypt, all claiming the title "king of Upper and Lower Egypt". We even cited one instance where as far south as Meroe the kings Ankhare and Khnemibre employed the identical title, though they ruled hundreds of miles south of Egypt. Clearly the designation "king of Egypt" was a formulaic and conventional epithet rather than a description of reality. And we see in the annals of Ashurnasirpal and Shalmanezer III that multiple kings in north Syria are entitled "king of Hatti", all in the identical time frame, including Sangara who elsewhere is clearly associated only with the city of Carchemish. Shalmanezer several times claimed to have crossed the Euphrates to collect tribute from these "kings of Hatti" (sharru ^{mat}Hatti). In another context 12 "kings of Hatti" are said to have assisted Hazael of Damascus in his wars with Shalmanezer. There is clearly no intention that the title "king of Hatti" be understood as an all encompassing term, as if the designate were the king of the Hittite Empire. In one instance only, that of Sapalulme, did the title mean precisely that. But in the other cases it implies only that the author was a Hittite or a Hittite representative and that he was a "king".within the Hittite Empire. The use of the title "king of Assyria" by the Amarna king Ashuruballit should be interpreted similarly and not construed as if he ruled over the whole of the country.[41]

We can assume therefore, barring evidence to the contrary, that the state of Bit-Adini in the 10th century was ruled by an Assyrian, whether or not a rival to either Ashurdan II or Adad-nirari II. He was at least the second generation of a family that governed the area.[42] This assumption can neither be proved nor denied, but considering the state of affairs in the

[41]We simply do not know how princes, ruling over various regions of the Assyrian empire, addressed themselves to foreign dignitaries in their correspondence. The Amarna letters are among the few examples of such dialogue in existence. It could well be that there were dozens of "kings of Assyria" governing simultaneously, just as there were multiple "kings of Hatti" in north Syria at any one time. It is inconceivable that Ashurnasirpal and Shalmanezer III would employ the title "king of Hatti" in such a cavalier manner to Hittite regional kings, yet demand that the title "king of Assyria" be employed more strictly.

[42]Even though Ashuruballit claims that his father Ashur-nadin-ahhe was sent gold by an earlier Egyptian king in response to a request, we need not assume that his father was acting independently, nor that he claimed the title "king of Assyria" (though he probably did). He may simply have been the governor of the Harran region, acting on behalf of the Assyrian king.

region early in the 9th century, it is perfectly reasonable. Furthermore, there is precedent for our suggestion that the rulers of this region self-styled themselves as kings, and thus as "kings of Assyria". Less than seventy years after Shalmanezer retook the region for the Assyrians, in the days of Shalmanezer IV, its governors were acting like "kings" in their own right, this according to the inscriptions of that day.

Even the central provinces maintained only a tenuous loyalty to Assyria, for the various governors ruled in virtual independence of the king at Kalah. One of them, Shamshi-ilu, inscribed his own monuments at Kar-Shalmaneser (Bit Adini) in quasi-royal style, and even after this fortress had to be abandoned he virtually ruled the empire as turtanu (commmander-in-chief)[43]

Even more to the point, another 150 years later still, when the last Assyrian king Sinsharishkun died, in or shortly after the fall of Nineveh, an Assyrian governor of Bit Adini, resident in Harran, proclaimed himself "king of Assyria" (though we assume that he already used the title), and was recognized as such by the Babylonians. That his name was also Ashuruballit, the second of the known kings bearing this name, is by no means irrelevant.. Perhaps the naming of this king was guided by some historical memory of events that took place in that region hundreds of years earlier, in the days of Akhenaten.

The Amarna letters themselves give some hint that the author of letters EA15and EA16 was not the head of the powerful state of Assyria (whether he lived in the 14th century B.C. or later). The Amarna Ashuruballit sends as a greeting gift a single chariot, two horses and a lapiz lazuli seal. He informs the Egyptian king that he is building a palace, and pleads for "gold, as much as its decoration and its requisites demand". The second letter in particular, concerned entirely with gold, is more the groveling of a minor potentate than an entreaty to an Egyptian king from an equally powerful Assyrian ruler. The letters from Mitanni and Karduniash (Babylon?), supposedly lesser kingdoms than that ruled by Ashuruballit I, speak of the giving of daughters in marriage to cement their relationship with Egypt. Nothing of the kind is envisaged for the Amarna king Ashuruballit.

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[43]Ibid, p. 131

We leave the matter there. In our opinion Ashuruballit, of el-Amarna fame, was "*a* king of Assyria", but not "*the* king of Assyria". Thus the omission of his name from the king lists. His proximity to Syria explains why he, and not Ashur-dan II or Adad-nirari II (909-889) corresponded with the Egyptians. The city of Ashur lay two hundred miles east of Harran, three hundred from the Euphrates and seven hundred from Egypt. The state of Bit-Adini governed by Ashuruballit extended to within 350 miles of Egypt, half the distance to Ashur.

We expect that excavations in the area of Harran will one day confirm our opinion that the Ashuruballit who authored EA15 & EA16 governed the trans-Euphrates region of the Assyrian empire in the last decades of the 10th century B.C. Regardless, the mere fact that an Assyrian governor/local king has employed the title "king of Assyria" in a solicitous letter to an Egyptian pharaoh should not be considered a linchpin in any chronological scheme, especially one which anchors a sequence of Egyptian dynasties hundreds of years out of place.

The Mitanni

The criticism will surely be raised that we have said very little about the Mitanni. Specifically it may be questioned why this national group, so prominent in the Amarna letters - thus in the time frame 930-900 B.C. in the revised history - and a constant source of concern for the Hittite Empire, is ignored by the Assyrian kings Ashurnasirpal (883-859 B.C.) and Shalmanezer III (858-824 B.C.), whose lands supposedly bordered on Mitanni territory.

The answer is not far off. It is well known from Hittite inscriptions, as well as from several Amarna letters, that Suppiluliumas, early in his career, launched a series of attacks on the Mitanni, then the dominant power in much of north Syria. It is assumed, but not proven, that the Mitanni, from their capital Washuganni about 150 miles north-east of Assur, controlled much of the land mass east of the Euphrates, their southern boundary running just north of Harran, clearly bringing them into conflict with the Assyrians both in the northern and western fringes of the Assyrian empire. In the first phases of a prolonged war the Hittite

king supplanted the Mitanni as the dominant power in North Syria; in the second he laid siege to and conquered Washuganni, driving Tushratta, the Mitanni king, north of the upper reaches of the Euphrates, where the Mitanni, greatly reduced in land area and strength, remained a threat to the Hittites of future generations, but were far from the Assyrian center of influence. There Tushratta was killed in an insurrection led by his son Kurtiwaza, who was in turn quickly supplanted by Suttarna. The fact that Ashurnasirpal and Shalmanezer do not mention the Mitanni is explained from an analysis of their annals. Their battles took them primarily to the west, where they competed with the Arameans (in particular with Ahuni) and the Hittites for control of the lands formerly contested by themselves, the Arameans and the Mitanni. Elsewhere, to the north they engaged in frequent wars with the Nairi lands south of lake Van, and to the north-east less frequently with the inhabitants of the region around Lake Urmia. The Mitanni, living in the remote regions of the north-west, were avoided.

The fact that the siege of Washuganni took place early, rather than late in the career of Suppiluliumas, thus probably in the first decade of the 9th century, is clear from an incident well documented and much discussed by scholars. During the siege of Carchemish (the last holdout in the Hurrian war), apparently dated several years after the fall of Washuganni, Suppiluliumas received an unusual request from Egypt. Goetze describes the event:

> While Carchemish was under siege and this second army stood at Amqa, news reached Shuppiluliumash that a pharaoh, whom our source calls Piphururiyas, had died. His identity has been much discussed, the publication of a new fragment in which the name is given as Niphururiyash finally decides the issue in favour of Tutankhamun, Akhenaten's son-in-law. According to the chronology followed in this work his death occurred c. 1352. A remarkable message from the pharaoh's widow was conveyed to Shuppiluliumash. It deserves to be quoted here in full: "My husband has died, and I have no son. They say about you that you have many sons. You might give me one of your sons, and he might become my husband.. I would not want to take one of my servants. I am loath to make him my husband." This offer was so surprising to the Great King that he called together his noblemen into council and decided first to investigate whether the request was sincere. A high official, Khattusha-zitish was sent to Egypt. During his absence in Egypt, Carchemish was taken by storm more quickly than anyone expected. CAH II Part 2:18

A second letter followed soon after the first, the Egyptian queen complaining "I have not written to any other country, I have written only to you ... He will be my husband and king in the country of Egypt." It is assumed by scholars that the writer of the letter was the widow of king Tutankhamon, who was desperate to find a husband other than Ay. As a result the incident is dated to the last years of the life of Suppiluliumas, since in the traditional history the Hittite king outlived Tutankhamon by only a few years. Thus the capture of Carchemish is dated to the end of the reign of Suppiluliumas, a rather surprising turn of events, since the balance of North Syria was supposedly conquered by the Hittites in the early years of his kingship, at least thirty years before.

We have a better explanation. Carchemish fell around the year 885 B.C., at most five to ten years after the conquest of the balance of North Syria and even less following the siege of Washuganni. Scholars are almost certainly correct that the Egyptian queen is the widow of Tutankhamon, but Ankhesenamun is not recently widowed. Her husband died in his late teens perhaps as much as twenty years before, perhaps longer. She is now in her late thirties, desperate to marry, but not wanting to lend legitimacy to the rule of Horemheb. Besides, Horemheb is by now an old man, now into the 43nd year of his extended "reign". Perhaps she anticipates his death. Thus the letter, which promises more than Ankhesenamun can deliver. The son of Suppiluliumas who was eventually sent was murdered en route to Egypt, probably at the initiative of Horemheb.

By 885 B.C. the Hittites are fully in control of North Syria. Only then, during the reign of Ashurnasirpal, do Assyrian inscriptions multiply and we become informed as to the state of affairs in Mesopotamia. We are not surprised that the Mitanni are no longer on the scene.

Epilogue

We have omitted, by design, the bulk of the supportive argument for our thesis. The excavation of hundreds of sites in the region of Anatolia, north Syria, Greece and the Balkans, et al. reveal a gap of between four and five hundred years in the archaeological and cultural records of the local inhabitants, the Mycenaean late Bronze age of the 13th century

immediately followed by the 9th/8th century iron age without apparent interruption in all locations. We have perused the archaeological records at only two such sites, Gordion and Carchemish. For a more detailed examination of the extent of the problem the reader should read the massive research produced by the *Centuries of Darkness* authors on this subject. Virtually everything written by these scholars lends support to our thesis.

Lest we be accused of circular reasoning we have not depended on the Egyptian synchronisms between Hattusilis and Ramses II in our argument (other than to fine tune our positioning of Hattusilis III). But these synchronisms are in fact confirming aspects of our analysis. All of the argument in our earlier two books and the beginning chapters in the present work, those which placed Ramses II in the years 840-774 B.C., must be understood as supportive of our dating of the Hittites. At no time in that developing argument did we rely on the revised dates for the Hittite Empire. And our placement of the Hittites in the identical time frame as the neo-Hittites depended primarily on Assyrian and Anatolian synchronisms, confirmed by the archaeology of the region. We might well have ignored the Amarna letters (the only link with Egypt that we have used) and still determined that the Hittites belong in the same general time frame as we have placed them, the 9th/8th centuries. Thus the 9th/8th century dating of Ramses II and the 9th/8th century dating of the Empire Hittites who were contemporary with Ramses II, as attested by multiple documents, could have been arrived at independently. The reader can believe, if he/she so chooses, that this is merely coincidence. But there must come a time when coincidences multiply to the extent that we can no longer avoid the obvious conclusion. The archaeological records at hundreds of locations in the near and middle east are not in error; it is the historical assumptions which guide their interpretation that are wrong. The Egyptian 19th dynasty and the Hittite Empire are not coincidentally linked to the 9th/8th centuries, the one by a mass of evidence which lowered the dates of the 22nd through the 26th dynasties by 121 years, then the 20th and 21st dynasties by upwards of 450 years to overlap the 22nd; the other by Assyrian and Anatolian inscriptions which place the two kings called Suppiluliumas and those kings intermediate between them squarely in the midst of the neo-Hittite period. They are linked to the 9th/8th centuries because that is where they belong.

Chapter 5
Ugarit & Troy

The Problem of Ugarit

Two distinct peoples dominated the north-eastern Mediterranean coastline during the heyday of the Hittite Empire, that is, in the time interval from Suppiluliumas I to Suppiluliumas II. The northern and central coastal regions, those which lay east of the Orontes and the Lebanon range and ran roughly from Beirut to Simyra, were part of the state of Amurru, well known from the Amarna letters when it was dominated by a troublesome tyrant named Aziru. Further north, extending to the Anatolian coastline, thus bordering the Hittite kingdom itself, lay the state controlled by the port city of Ugarit.

Our interest in Ugarit in the next chapter is restricted to its final days in the middle of the 8th century. Here our attention remains focussed a century earlier.

In the last chapter, when we discussed at some length the infamous march by Shalmanezer III (858-824 B.C.) across the Hatti lands of north Syria to the Mediterranean coast, whence he "washed his weapons in the sea", we omitted any reference to a potential problem for our thesis. In our revised history the incident took place in Shalmanezer's initial year as king (858 B.C.) and was followed up by a second whirlwind tour of conquest, which again extended to the Mediterranean, in his 4th year (855 B.C.) In our revised history these dates correspond to the initial years of the reign of Mursilis. The city of Ugarit must have been a thriving metropolis at the time, yet Shalmanezer makes no mention of its existence. The critic will surely ask why.

A closer look at the annals of Shalmanezer provides the answer. In his initial tour of conquest Shalmanezer's advance to the sea took him considerably to the south of Ugarit, where he apparently had no interest in engaging the peoples of the area in battle. The Assyrian king expressly states in his annals that the cities of the coast offered no

resistance. Consequently they were left to themselves and are not named in his records.

> "I received tribute from the kings of the seashore. I marched straightaway unopposed ... throughout the wide seashore", i.e. along the coast. ANET 278

As the annals continue it is apparent that the Assyrian army at the time moved southward, away from Ugarit.

The situation was different in Shalmanezer's fourth year. This time several coastal cities resisted the advance of the Assyrian army and were brutalized in response. Again Ugarit is not mentioned by name, but this time its presence is confirmed, albeit obliquely. Explaining precisely how requires us to first produce, in summary form, a listing of the city's kings during the final century of its existence.

Kings of Ugarit

A listing of the known kings of Ugarit during the 9th/8th centuries will serve to support our revised chronology in three ways. On the one hand it will allow us to argue our claim that the city continued to thrive at least as late as the 4th year of Shalmanezer III, thus through the middle decades of the 9th century B.C. This fact, if it can be proved, contrasts markedly with the view of the traditional history in which Ugarit was destroyed by a combination of marauders and natural disaster very late in the 13th century B.C. The king list will also establish with some degree of certainty that the dates we have provided for Mursilis are accurate within a few years.

Finally, our analysis of the king list will serve to validate a proposal we made earlier to considerably reduce the reign lengths of the Hittite kings Mursilis and Muwatallis, what some might have considered at the time to be an arbitrary and self-serving adjustment to the historical record.

We begin with the latter issue. But first the king list.

Reign Lengths of the Kings of Ugarit

Following the accidental discovery of the site of Ugarit (Ras Shamra) early in the 20th century, multiple excavations provided a treasure trove of inscriptional material, much of it dating to the last century of the city's existence, the majority related to the final traumatic days before the city fell to the combined onslaught of invading "sea peoples" and catastophic earthquakes. Synchronisms between Hittite and Ugaritic kings provided by these materials allow scholars to establish the order and a rough chronology of the terminal Ugaritic kings, from the days of Suppiluliumas I to the fall of Hatti in the time of Suppiluliumas II.

According to several documents retrieved from the "rubble" of the city, the destruction of Ugarit and the fall of Hattusas, the Hittite capital, must have occurred within months, if not days of each other, an event we date to the middle of the 8th century, and discuss at length in the next chapter. Our attention here is focussed on the middle of the ninth century, the time of a Ugaritic king named Niqmaddu and his sons Arhalbu and Niqmepa, who are clearly identified in the archives of Ugarit as contemporaries of Suppiluliumas I and his immediate descendants.

Since the traditional history dates Suppiluliumas I and his immediate offspring to the 14th century B.C., it is compelled to date Niqmaddu and his sons to that same time frame. We have previously listed the traditional dates for the Hittite kings. The table below adds the approximate dates assigned by the traditional history to the kings of Ugarit for the same time period. Comment follows.

Table 11: Traditional History Dates for the Kings of Ugarit During the Time of the Hittite Empire

Hittite kings	Traditional Dates	Kings of Ugarit	Traditional dates.
Suppiluliumas I	1375-1335	Amishtamru I	c. 1360
Arnuwandas III	1335-1334	Niqmaddu II	1360-1330
Mursilis II	1334-1306	Arhalbu	1330-1324
Muwatallis	1306-1282	Niqmepa	1324-1265
Urhi-Teshub (Mursilis III)	1282-1275	Amishtamru II	1265-1240
Hattusilis III	1275-1250	Ibiranu	1240-?
Tudhaliyas IV	1250-1220	Niqmaddu III	?-1225
Arnuwandas IV	1220-1200	Amurappi	1225-1180
Suppiluliumas II	?	"	

The dates assigned to the kings of Ugarit vary considerably from scholar to scholar, depending in part on the dates assigned to their Hittite contemporaries. However, several aspects of the chronology are invariable and need to be highlighted.

While the length of the reign of the king known as Niqmaddu II is unknown, two aspects of his tenure in office are generally accepted. His reign overlapped to a large extent the end of the reign of Suppiluliumas I, with whom he is known to have entered into a treaty relationship. The document describing Hittite suzerainty over Niqmaddu's realm is extant. It is also generally agreed that his reign extended into the early years of Mursilis II, ending around the 4th or 5th year of this Hittite king.

There is also general agreement that Niqmaddu II was succeeded by two sons, Arhalbu and Niqmepa. The first son ruled for less than a decade, his reign ending mysteriously as the young king, forseeing his end, bequeathed his wife to his younger brother Niqmepa. Perhaps he was dying of the plague which at the time ravaged the Hittite kingdom of

Mursilis. His brother was more fortunate. Not only did he live to rule a normal span of years, but enjoyed a prolonged reign of almost 60 years, an abnormal longevity to say the least. And therein lies a problem.

The extended reign of Niqmepa is the stuff of legends. Documents confirm that his rule overlapped the reigns of Mursilis, Muwatallis, Urhi Teshub and Hattusilis. And all this after he succeeded not his father, but his brother! A cursory glance at the table above suggests that the combined reigns of Niqmaddu II and sons lasted for 95 years. Remarkable if true! Amazingly, scholars do not seem to have a problem with the suggestion. In her *Cambridge Ancient History* article on "Ugarit" Margaret Drower merely cites the data and moves on.

> After a long reign of perhaps more than sixty years, as the vassal of four successive Hittite sovereigns, Neqmepa was succeeded, about 1265 B.C. by his son Ammishtamru, the second of the name. CAH II.2 p. 141

But the difficulty with the chronology does not end here. Drower goes on immediately to document the reign of Niqmepa's son Amishtamru II.

> Although he must have been a middle-aged man at the time of his accession, it would appear that the affairs of the state were managed for a short time by the dowager queen Akhat-milki, the daughter of King DU-Tessub of Amurru. (p. 141)

Dower is mistaken. There is really only one way to interpret the documents which form the basis for her otherwise innocuous statement. Amushtamru was not, as stated, "a middle-aged man at the time of his accession". He was clearly not old enough to reign when his father died, and the kingdom was for a time ruled in his stead by his mother. But interpreting the evidence in this most straightforward way would only serve to compound the chronological difficulty. It is already necessary to assume that Niqmepa (who was already an adult close to thirty years old when he inherited the throne from his brother) was well over ninety when his reign ended. The suggestion that he fathered a child by his Amorite wife only a decade or so before his death only serves to exacerbate the problem. While not impossible, the scenario is unlikely. We believe that a faulty chronology is the likely cause of the problem.

In the revised history the situation improves considerably. With the reigns of Mursilis and Muwatallis overlapping and considerably reduced in length in the revised history (as we argued in the last chapter), the reign length of Niqmepa can be reduced to about half the traditional value. The dates in the table below, although approximations only, are probably accurate to within five years.

Table 11: Revised History Dates for the Kings of Ugarit
During the Time of the Hittite Empire

Hittite kings	Revised Dates	Kings of Ugarit	Revised Dates.
Suppiluliumas I	908-858	Amishtamru I	
Arnuwandas III	858-857	Niqmaddu II	883-855
Mursilis II	857-836	Arhalbu	855-848
Muwatallis	846-832	Niqmepa	848-815
Urhi-Teshub (Mursilis III)	832-825	Amishtamru II	815-790
Hattusilis III	825-800	Ibiranu	790-?
Tudhaliyas IV	800-775	Niqmaddu III	?-775
Arnuwandas IV	775-765	Amurappi	775-760
Suppiluliumas II	765-760		

The Expulsion of Niqmaddu II.

The history of Ugarit under discussion relates to our thesis in yet another way, previously mentioned. In both the traditional and revised histories there is consensus that the reign of Niqmaddu II ended in a bizarre fashion sometime early in the reign of Mursilis II. The two historical schemes differ only in the absolute dates assigned to the incident, and thus in the interpretation of the relevant data. Since much of the discussion which follows is not original to this author, and is borrowed

from the earlier revisionist work of Immanuel Velikovsky, we use that author's words to introduce the relevant documents.

In the fifth chapter of his *Ages in Chaos* Velikovsky describes an inscription found in the ruins of Ugarit which he relates to the time of the final destruction of the city.

> The excavator of Ras Shamra found that the city had been destroyed by violence and had not been rebuilt. Buildings were demolished; the library burned and its walls fell on the tablets and crushed many of them. The last king whose name is mentioned on the documents that survived the fire is Nikmed. There was also found a proclamation which states that the city was captured and Nikmed and all the foreigners were expelled. *Ages in Chaos* 219-220

We are extremely interested in the proclamation referred to by Velikovsky, but before we describe its contents we should set the record straight regarding its context as interpreted by the famed revisionist. By this stage in his revision Velikovsky has already made two serious errors regarding a king named Niqmed (Niqmaddu). In the first place he wrongly assumes that the Nikmed whose name is mentioned on many documents which survived the destructive fire, who must therefore be Niqmaddu III of the king list provided above, was the last king to govern the city before its final destruction. Other documents clearly demonstrate that this dubious honor belongs to Ammurapi, the son of Niqmaddu III. Secondly, Velikovsky assumes, incorrectly as it turns out, that the proclamation inscription he proceeds to describe belonged to this same Niqmaddu III. We argue instead that it was inscribed in the last days of Niqmaddu II and was subsequently preserved in the city archives. Having voiced our objection we allow Velikovsky to continue.

> The proclamation found in Ras Shamra is directly related to the upheaval in the life of the city. Some invading king decreed that "the Jaman [Ionians], the people of Didyme, the Khar [Carians], the Cypriotes, all foreigners, together with the king Nikmed" were to be expelled from Ugarit, "all those who pillage you, all those who oppress you, all those who ruin you." *Ages in Chaos* 221
> [Velikovsky is here quoting a document translated by the Hittite linguistic pioneers Hrozny ("Les Ioniens a Ras-Shamra," Archiv Orientalni, IV (1932), 171) and Dhorme (Revue Biblique XL (1931) 37-39)]

The proclamation referred to was apparently addressed to the Phoenician element of the city's population in the standard Ugaritic of the day

(cuneiform Semitic), and was authored by some unknown invading monarch. Hrozny, who first translated the document, expressed the opinion that the king who erected the inscription was perhaps Babylonian, this based on several hints in the text. He also expressed remorse that the opening lines of the text were missing, a sentiment echoed by Velikovsky.

> The opening portion of the proclamation is missing; this is regrettable because it might have revealed the name of the king who expelled Nikmed. Who was the king who conquered Ugarit, burned it, expelled its population, and caused King Nikmed to flee? *Ages in Chaos* 221

Velikovsky promises to identify the king in a later chapter. We follow his identification with interest. As it turns out, he is correct. But unfortunately for Velikovsky, who continues throughout to identify the Niqmed in the proclamation as Niqmaddu III, and steadfastly maintains the belief that this same Niqmed ruled during the last days of Ugarit's history, the chronology which he proposes must be rejected.

In the revised history there can be no doubt as to the identity of the king who deposed Niqmaddu II. According to the revised dates for the Ugaritic kings proposed earlier, Niqmaddu II yielded the kingship to his son 'Arhalbu around the year 855 B.C. Those dates were not created ad hoc in order to provide the synchronism we now propose. They are simply the traditional dates of the Ugaritic kings relative to the dates of the Hittite Empire kings as revised earlier. It is therefore most significant that Niqmaddu's reign ended around the 4th year of Shalmanezer III, the precise date when, according to Shalmanezer's annals, the Assyrian king invaded Ugaritic territory for the second time. Even if we had no further evidence to support our contention, we would strenuously argue on the basis of this chronology that Shalmanezer III must be the conqueror of Ugarit and thus the foreigner who deposed king Niqmaddu II. But further evidence does exist. Again we listen again to Velikovsky, who first proposed the identification under discussion and found reference to Niqmaddu in the Assyrian annals.

> In the section "End of Ugarit," in Chapter V, we asked, Who was the invader? We quoted also a proclamation found in Ras Shamra-Ugarit in which the invading king decreeed that "the Jaman [Ionians], the people of Didyme, the Khar [Carians], the Cypriotes, all foreigners, together with the king Nikmed," should be expelled from Ugarit. The opening portion of the proclamation, which

> might have revealed the name of the king who expelled Nikmed, is missing. Some intimation has been found that Nikmed was expelled by the Babylonians .... This suggestion is not far from the truth, since Babylon was incorporated by Shalmaneser into his empire.
>
> We are curious to know whether Shalmanezer left any written record of his conquest of Ugarit. And in fact we find the following entry twice repeated in his annals: "Year four: To the cities of Nikdime [and] Nikdiera I drew near. They became frightened at my mighty, awe-inspiring weapons and my grim warfare, [and] cast themselves upon the sea in wicker[?] boats ---, I followed after them in boats of ----, fought a great battle on the sea, defeated them, and with their blood I dyed the sea like wool. [Luckenbill, Records of Assyria, Sec 609]" *Ages in Chaos* 309

In spite of many mistaken notions regarding the historical positioning of Ugarit in his revised history of the ancient near east, Velikovsky is in this respect to be credited with a profound insight. The city of Nikdime in the Assyrian records must, in context, refer to a coastal city in the vicinity of Ugarit. The year is 855 B.C., the 4th year of Shalmanezer III. According to our revised history it is also the year the reign of king Niqmed II ended at the hands of an invader. The proclamation found in the ruins of Ugarit maintains that king Niqmaddu was deposed by an invading king who had some east Semitic (Babylonian) affinities. Shalmanezer III was king of Assyria and Babylon. Let the reader decide what should be made of this sequence of "coincident" facts.

We should not be confused by the inversion of two elements in the names Nikmed (Niqmaddu) and Nikdime. As Velikovsky correctly argues:

> The city of Nikdime appears to be the city of Nikmed. Cities were named in honor of their kings, and in this case it is put clearly, "of Nikdime." The translator of this record also explained the words "city of Nikdime" by a gloss; "personal name." The inversion of two consonants, especially in personal names of foreign origin, is very common among oriental peoples. *Ages in Chaos* 309

We leave the matter there and move to the 8th century.

The Emergence of Troy

Shalmanezer III (858-824 B.C.) warred with Syria for much of his reign. As late as his 18th year (841/40 B.C.) he fought against Hazael of Syria, who several years earlier had succeeded Ben-Hadad of Damascus as the major domo of the southern Syrian confederacy. This would be his final razzia through Syria, though his reign continued for another fifteen years. One of the reasons for this abrupt end to hostilites was a growing insurgency within Assyria itself, civil conflict which continued through the reigns of Shalmanezer's successors well into the 8th century. Some of this conflict we documented in the last chapter. Assyria would not reemerge as a factor in Syrian politics until the second half of the 8th century with the arrival of the military genius of Tiglath Pilezer III.

A second reason for Assyrian isolation from Syria during and following the terminal years of Shalmanezer III was the reassertion of Egyptian power in the Levant under Ramses II (840-774 B.C.), this and a continued, if not heightened, Hittite influence in the region under a succession of powerful kings, in particular Hattusilis III (825-800) and Tudhaliyas IV (800-775). During this time Hatti and Egypt vied for control of Syria to the exclusion of the Assyrians. By this time also other peoples had entered the picture, two of particular interest to this revision.

If our revised history is correct, and the Hittite king Tudhaliyas IV lived in the early decades of the 8th century, not in the late 13th century where he is positioned by the traditional history, then we are compelled to argue that this same time frame witnessed the emergence on the historical scene of two peoples of great significance for our revision. On the western Anatolian coast the Trojans are mentioned for the first time in Hittite documents contemporary with Tudhaliyas IV, appearing first as allies, then as adversaries of the Hittites. Across the Aegean the Achaeans (Mycenaeans) emerge simultaneously, again as both friend and foe of the Hittites. We are clearly within the Mycenaean age, nearing the time of the mamoth conflict between the Achaeans and Trojans, the Trojan war of Homeric legend. All this, of course, if our revised chronology is accurate.

The Swiss linguist Emil Forrer was the first to observe, early in the last century, in the newly discovered archives of the Hittites, not only

references to place names such as Taruwissa (Troy), Achiyawa (Achaea), and Wilusiya (Ilios), but also instances of the personal name Alaksandus (Alexander-Paris) and the Hittite equivalents of Atreus, Eteocles, and Andreus. Since the days of those initial observations, contested at the time, scholars have been increasingly accepting, albeit at times reluctantly, of the fact that the Trojans (and Mycenaeans) belong in the same chronological time frame as the late Empire Hittites. The evidence compels us to reach that same conclusion. But for the revised history the time frame is the early 8th century, not the late 13th. Our chronological positioning of Tudhaliyas IV in the early 8th century is an inevitable consequence of the entire argument of the first two books, and the earlier chapters of the present book of our historical revision. We have no choice in the matter. We are obliged to date the Mycenaeans and Trojans in the same 8th century context as Tudhaliyas. It follows that all of the evidence, both literary and archaeological, that supports an early 8th century rather than a late 13th century date for the Trojans and the Achaeans, must be viewed as supportive argument for our thesis. And the evidence is widespread and compelling.

We do not have to go far afield to argue that the Trojans and the Trojan war belong in the first half of the 8th century. The belief that the Mycenaean age celebrated by Homer immediately preceeded the late 8th century classical Greek period was the prevailing view among scholars of the early 19th century, before the Mycenean age was unceremoniously thrust hundreds of years back in time by Egyptian scholars. This opinion held regardless of whether or not the Trojan war was considered fact or fiction. It was based on two separate strands of evidence, namely 1) the massive amount of archaeological and cultural evidence which shows clearly that the classical Greek period which began in the late 8th century follows the Mycenaean age without interruption; and 2) the incontrovertible evidence from documentary sources, primarily but not exclusively the literature related to the Trojan war, that the story of the conflict between the Mycenaeans and Trojans was composed late in the 8th century, and if so, then the events which gave rise to the epic narrative must be recent. Volumes of literature have been devoted to both themes over the past centuries, and the argument continues.

Several times already we have made reference to the first of these lines of reasoning. We have argued that the so-called "dark ages" of Greece were created by sleight of hand when scholars, based exclusively on a faulty Egyptian chronology, mistakenly moved the Mycenaean age backward 450 years, creating an artificial historical vacuum which has subsequently wrought havoc with the archaeological and cultural historical records of all countries whose past intertwined with that of Egypt. Archaeology argues strongly that the Mycenaean age ended around the middle of the 8th century. Volumes could be written in defense of that claim.

The documentary problem is no less acute. When Homer and other authors of the late 8th century mythologized events related to the Trojan/Achaean conflict which had taken place only decades earlier, they inevitably left a datable cultural footprint in the language they used and the events and artifacts they described. Scholars have always been all but unanimous in arguing that the final editing of the Iliad and the other literature related to the Trojan war is late, arguably composed in the late 8th century. Common sense dictates that the events described therein antedate the composition of the story by decades, not centuries. It was only when the Mycenaean age was moved from the 8th century to the 13th century that scholars were compelled to invent a literary "dark age" to mirror the artificial gap that had been created in the archaeological record. Thus the invention of a fictional 450 year long period of time during which the story of the Trojan war was transmitted "orally", embellished all the while by an accumulation of "anachronistic" material, until finally being fixed in its present form in the late 8th century. We will waste no time discussing the flaws in this "oral tradition" thesis. Let the reader browse the literature on the subject to see if it makes any sense.

We leave the matter there. Sufficient to note that when the revised history places the Trojans and the Trojan war in the 8th century it does not encounter evidence to the contrary, but rather a mass of supportive linguistic and archaeological data, and this for an understandable reason. It is merely returning the Mycenaeans and the Trojans to their former and rightful place in history.

We make but a single exception to our decision to bypass the archaeological/cultural/literary evidence supportive of our dating of the Trojan war - namely, the excavation record of Troy itself.

The Ruins of Troy

The reader interested in the "dark age gap" in the archaeological record within the Greek world is referred once again to the excellent compendium on the subject in the recently published *Centuries of Darkness* authored by a group of Cambridge scholars. In particular the third chapter - "Beware of Greeks Bearing Gifts" - and the fourth chapter - "The Dark Age Mysteries of Greece" - of this excellent work are strongly recommended reading. We use the words of the authors Peter James et. al. from this source to draw attention to the central problem at Troy, a problem related to the consensus view of scholars that Homer's Troy must be identified with either strata VI or VIIa of the excavated city site.

> The sack of Troy by the Greeks under the command of Agamemnon, King of Mycenae, has lauched a thousand articles. The search for the city of Homer's Iliad will certainly continue to fascinate archaeologists for years to come. Schliemann correctly identified the massive mound of Hissarlik as the site of Troy, but which of the superimposed cities he found should be associated with the time of the Trojan war remains unclear. The rival candidates are currently the cities of Troy VI and VIIa. While the earlier city and its magnificent defensive walls provide a better match with Homer's account, it was apparently destroyed by an earthquake and predates the high point of Mycenaean power in the 13th century BC. The latter city was burnt down but its poor remains fail to measure up to the description of the mighty city of Priam given in the *Iliad*. (CD 60)

We should note at the outset that strata VI, VIIa and VIIb of ancient Troy are all clearly dated by archaeologists within the Mycenaean period, though the duration of occupation in the respective periods is open to question. It is entirely possible that only a few years separate the destructions by earthquake and by fire of levels VI and VIIa. The question as to which level corresponds to the occupation and conclusion of the Trojan war is a difficult one to answer, since the argument in part involves the reliability of the authors of the epic literature, and we simply do not know what literary license should be accorded them. All we can

glean from the facts at hand are that the combined ravages of earthquake and war contributed to the demise of the glory that was Troy. We will see this same duality - earthquake and war - occurring at other sites around this same time period. The two phenomenon invariably occur in sequence and, as we will argue later, are probably related. It is always the same - physical calamity (earthquake) followed by warfare and fiery destruction.

The real stratigraphic problem at Troy is not whether level VI or VIIa is the occupation level of the beseiged city of Priam and Paris, rather it is whether these Mycenaean strata belong to the 13th or the 8th centuries. We let Peter James continue:

> This problem aside, the site presents other difficulties that are just as challenging. Despite numerous excavations, no strata have yet been discovered representing the period between Troy VIIb, usually linked with 12-century Mycenaean imports (LHIIIC), and the beginning of Troy VIII, dated by Archaic Greek imports to 700 B.C. The classical scholar Deny Page remarked on the strange gap which results from this chronology:
>
> > There is nothing at Troy to fill this huge lacuna. For 2000 years men had left traces of their living there; some chapters were brief and obscure, but there was never yet a chapter left wholly blank. Now at last there is silence, profound and prolonged for 400 years; we are asked, surely not in vain, to believe that Troy lay 'virtually unoccupied' for this long period of time. (CD 61)

The problem does not end there. It is not simply a matter of explaining why a classical Greek site of the late 8th century (level VIII) lies immediately above a Mycenaean site of the 13th century (level VIIb). Rather it is a matter of explaining the apparent *continuity* which exists between the levels VIIb and VIII, this on the assumption that a 400 year hiatus has occured in the occupation of the two levels. Again we let Peter James explain:

> Yet despite the apparent lapse of several centuries, there is every indication of continuity between Troy VIIb and VIII. The excavator, Carl Blegen, could detect no sign of a break in occupation. Furthermore, the local pottery of Troy VIII was the same distinctive, lustrous grey ware used during Troy VIIb. He therefore supposed that the inhabitants of Troy VIIb abandoned it for a nearby refuge, where they continued to produce this 'Grey Minyan' pottery for 400 years before returning .... Despite twenty-five years of further research in the area, no sign of Blegen's hypothetical refuge site has ever been uncovered. (CD 61-62)

There is, of course, no need to reconcile the disparate evidence at the site. None exists. With the Mycenaean age move forward into the 9th/8th centuries a plausible explanation for the respective strata is readily at hand. Level VI at Troy most likely represents Priam's city prior to a destructive earthquake which levelled much of its defensive fortifications around the year 765 B.C. Level VIIa is the occupation level of the surviving Trojans through the duration of the Trojan war, roughly spanning the years 765-755 B.C. The Trojan war (and thus level VIIa) ended with the burning of the city. Level VIIb was occupied by the survivors of the war, a mixture of Mycenaean Greeks and Trojan peasantry. It apparently lasted through the balance of the 8th century. Level VIII followed without temporal interruption. There is no 400 year gap in the stratigraphy of Troy. It is not the archaeological record which is inconclusive. At fault is the errant chronology which guides the interpretation of the data.

If we are correct in our appraisal of the situation there may well have been a cause and effect relationship at Troy between the destruction by earthquake and the destruction by fire. We surmise that it was the earthquake destruction and the resulting weakening of the defensive fortifications of the city, not the abduction of Helen of Troy by Agamemnon of Mycenae, that provided the motivation for the Greek assault on the city of Troy.

But what caused the earthquake?

The Santorini Explosion

When we assign tentative dates of 765 B.C. and 755 B.C. to the destructions by earthquake and fire of Troy VI and VIIa we are admittedly only guessing. The fact that the names of the participants in the Trojan war occur in documents from the time of Tudhalias IV suggests only that this reign immediately preceded the great war. The determination made by the latest excavators of Troy, that level VIII dates from late in the 8th century, provides a *terminus ad quem* for that same war, supported by the fact that the Homeric age which postdates the war is reasonably securely situated in that same late 8th century time

frame. Having said that, we are certain that the dates are accurate within five years. Our confidence is undergirded by evidence from elsewhere in the Near Eastern world, where these same two elements, destruction by earthquake and fire, occur in near succession in roughly the same time frame. It is imperative that we examine each occurance of these tandem destructions as they occur elsewhere in continguous cultures in the same time frame - Hattusas, Ugarit, Syria, Assyria, Israel and Egypt. It is the combined weight of evidence from each of these locations that suggests they are part of a common phenomenon. The task will be assigned to our concluding chapter. But before we begin we should state explicitly what we are looking for.

The fact that a series of natural disasters followed by attacks by marauding warfaring peoples occur in the same time frame in geographically remote regions of the Near East suggests both a common cause for the natural disasters and a similar cause and effect relationship between the disasters and the ensuing mass migration of warfaring peoples. Whether we are talking about the marauding hordes who dealt the final blows to Ugarit, the rebels who plagued Syria in the days of the prophet Amos, or the sea peoples who invaded Egypt for the first time under the successors of Ramses II, we are dealing with the same phenomenon. All of these "migrations" took place in conjunction with a series of widespread natural disasters, which suggests that the participants were opportunists, taking advantage of a devastation first wrought by nature. The natural disaster precipitated the warfare. We cannot prove this to be the case, but the assumption is reasonable.

A second conclusion can be reached from the material we are about to examine. If we are correct that the natural disasters occurred in the identical time frame in regions separated by many hundreds of miles, then the common cause must have been some catastrophic event - and if localized (a reasonable assumption) then it must have been centered in the Mediterranean, perhaps on or near the island of Crete. How else can we explain the facts, soon to be presented, that its effects reached as far south and east as Lybia and Sicily/Sardinia (from which rebels were driven to invade Egypt) and as far north and east as Hattusas and Assyria (where ash blocked out the sun for months). Countries bordering on the eastern Mediterranean and the Aegean, i.e. Achaea, Troy, Ugarit, Syria, Israel,

Egypt, Lybia, Sicily, were all either recipients of or participants in the resulting widespread aggression of warfaring peoples.

While the cause of these widespread natural disasters must ultimately remain undetermined - since we have no certain proof of either its nature or its location - we venture to propose one possible, if not probable source, needless to say one fraught with controversy. Tentatively we suggest that the culprit we seek was the volcanic island of Santorini, known also as Thera, supposedly destroyed in an eruption of unprecedented intensity sometime in the middle of the second millenium B.C. It should not surprise the reader of this revision that we claim that the dating for the explosive extinction of this volcanic isle needs to be moved forward in time hundreds of years, along with the Egyptian 18th dynasty which preceded it in time. That the explosive destruction of Santorini should still dated as late as the mid-8th century is perhaps surprising, but not beyond reason. A brief history of the island is in order before we proceed.

The Eruption of Thera

The island known as Santorini lies approximately 75 miles (125 km) north of Crete. According to the traditional history it was an important constituent of the Cretan Minoan confederacy until that civilization ended, supposedly in the early part of the 15th century B.C., at a time when Egypt was ruled by its 18th dynasty. The island, known also by the name Thera, was volcanic, its central peak rising to a height of around 5000 feet (1600 m) According to prevailing scholarly opinion a series of eruptions, culmating in a cataclysmic explosion, destroyed a major part of the island around the year 1470 B.C. The explosion not only destroyed a major part of the island, including much of the Minoan population both there and on Crete, but so weakened the Minoan civilization that it soon succumbed to an invasion of Mycenaean Greeks and vanished from history.

The final volcanic eruption of Thera is the stuff of legends. The explosion has been favorably compared to that of Krakotoa, east of Java in 1883 of the present era. That recent massive upheaval send giant tidal

waves throughout the south pacific and filled the atmosphere with ash that spread throughout the world, influencing climate for generations. Santorini, according to the experts, "was about 4 times larger than Krakotoa, and probably at least twice as violent. The fury of Santorini's final explosion is inferred from geologic core samples, from comparison to the detailed observations made on Krakotoa in 1883, and from the simulaneous obliteration of almost all Minoan settlements." One internet source summarizes the event as follows:

> In summer, circa 1470 BC, Santorini exploded. Volcanic ash filled the sky, blotted out the sun, and triggered hail and lightning. *A heavy layer of volcanic ash rained down over the Aegean*, covering islands and crops. *Earthquakes shook the land*, and stone structures fell from the motion. When the enormous magma chamber at Santorini finally collapsed to form the existing caldera, enormous tsunamis (tidal waves) spread outward in all directions. The coastal villages of Crete were flooded and destroyed. The only major Minoan structure surviving the *waves and earthquakes* was the palace at Knossos, far enough inland to escape the tidal waves. But in the days that followed, volcanic ash covered some settlements, and defoliated the island. Buildings were completely covered in volcanic ash by the cataclysmic explosion. In famine from the ash, with the bulk of their civilization washed away, the remaining Minoans were overrun by Mycaeneans from Greece, and Knossos finally fell. www.angelfire.com/hi/alhawk/atlanthira.html (italics added)

What was left in the aftermath of the great explosion was the jagged edge of the once proud and majestic volcano, the central caldera now covered by the Aegean sea, the rim less than a third the height of the original peak. A quick glimpse of the site immediately raises a question: What must have been the effects on contiguous areas of the ancient near east, indeed on the world, as this mountain turned to ash polluted the atmosphere and obliterated the sun for days and weeks or even months and years following? The event must have had severe consequences beyond those already noted for the Minoan and Mycenaean civilizations. But when precisely did this monumental event take place?

In spite of multiple claims to the contrary, it is not known when the Minoan civilization ended, nor precisely when the Mycenaean civilization began, even within the chronological schema proposed by the traditional history. Certainly the archaeological results from excavations on Crete provide no definitive answer. Santorini adds little to the overall picture. From excavations at Acrotiri, a Minoan center of population on

the rim of Santorini, artifacts linked to the Egyptian 18th dynasty have been found, as they have also on Crete. But that only proves that the destruction at Acrotiri, and the demise of the Minoan civilization, postdates the beginning of the 18th dynasty, and for the revised history that implies that the date of the Santorini eruption must lie after the beginning of the first millenium B.C. If it is dated early in the 10th century, where we have positioned the beginning of the 18th dynasty, then we can cease regarding the Santorini eruption as the cause of the natural disasters that jarred the Mediterranean world in the 8th century B.C. Even if that should be the case, all is not lost, since our discussion has focussed attention on what we believe was the ultimate cause of the 8th century phenomenon. We would simply have to focus our attention elsewhere in the Mediterranean world for a gigantic volcanic eruption of comparable size.

Having said that, we protest strongly that we are not compelled to date the end of the Minoan civilization during the early 18th dynasty, nor are we obliged to accept the claim that the massive eruption that destroyed Santorini actually caused the end of the Minoan civilization. There is absolutely no evidence that either statement is true. It should be noted in this regard that seismic activity at Santorini did not cease with the massive upheaval which destroyed much of the island mountain. Other eruptions have taken place well into modern times. Nor was the massive upheaval the first eruption at the site. Even the experts claim that the inhabitants of the volcanic island were well aware of the vagaries of their mountain and that it had been violently active for decades before the great explosion. Many inhabitants had vacated the cities before the massive eruption occurred. The volcano may well have had violent and destructive phases over the previous several hundred years. It is entirely possible that the experts have misconstrued the evidence, and that the massive upheaval of which we have been speaking followed the destruction of Acrotiri (the result of an eruption of lesser intensity) by upwards of two hundred years. It simply does not follow that all of the cities excavated from the rubble of the island were destroyed at the same time. The matter needs further looking into, but not at this time by this author. Such an investigation lies far beyond the purvue of this historical reconstruction.

We continue to believe that subsequent research will support our initial suggestion that Santorini was the epicenter and ultimate cause of the events we now set about to describe.

We also assume throughout the following discussion that the Thera volcano was active decades before its violent eruption around the year 765 B.C., causing widespread alarm and destruction in countries bordering on the the Mediterranean. The prophets in Israel warned the population of impending doom in the days immediately preceding the great upheaval. Merenptah in Egypt sent famine relief to Hatti as its climate apparently changed and crops failed. Fear gripped the near eastern world. Then came the explosion to end all explosions.

We believe that the earthquake which destroyed Troy VI was a side effect of the great Santorini eruption, though it may well have been triggered by an earlier outburst. If so, then the eruption must also have wrought havoc on the eastern shores of Mycenae, destroying crops and property, disrupting social stability, and precipitating aggressive military action, such as that directed against Troy. It remains to be seen whether the eruption that destroyed Acrotiri and other Minoan cities and the military action that wiped out the remnants of Minoan civilization (assuming the historicity of that event) belong to this same time period or resulted from an earlier phase of volcanic activity at Santorini. In 765 B.C. we are barely 100 years removed from the end of the 18th dynasty. The end of the Minoan culture must be located sometime in this relatively narrow temporal space. More than that we cannot say.

It is time to look elsewhere for corroborative evidence.

Chapter 6
Sea Peoples & Natural Disasters

Hatti

Scholars are not agreed on precisely how the Hittite Empire ended, although the consensus view holds that invaders were responsible, and further, that the invading peoples are perhaps the same mixture of warfaring Sea Peoples that attacked Egypt in the days of Merenptah and menaced Egypt in the days of the 20th dynasty king Ramses III.

The scholars are undoubtedly correct, at least in their belief that invading hordes dealt the coup-de-grace to the Hittites. And it is almost certain that the death blows were administered around the year 760 B.C. in the revised history. We have argued previously that the Hittites were succeeded immediately by the Muski/Phrygians ruled by a king Midas, perhaps but not certainly the king who later became a legend. According to our earlier reasoning this king began his rule around 765 B.C., the time of the Santorini explosion. The 760 B.C. date we have assigned to end of the Hittite empire falls only five years later. It is a date supported by the entire weight of argument in the three books dedicated to this revision. The Muski were at least the beneficiaries, if not the immediate cause of the Hittite collapse.

We cannot credit the invaders totally for the Hittite collapse. There is no doubt that some, if not most of the destruction inflicted on Hittite cities was caused by warfare, but we must point out that the collapse of the walls of Hattusas, and the havoc wrought elsewhere at the bend of the Halys, suggests that natural causes, including volcanic activity and earthquake, was a contributing factor. Most Hittite sites show evidence of destruction by fire. They are inevitably found buried in a layer of ash. But this does not necessarily imply warfare. As we will soon observe in the case of Syria, where destructive fire fell from heaven around this time, much of the burning may have resulted from molten cinder and ash falling from the sky, not the deliberate action of conquering hordes We wonder how many Hittite cities were burned and buried the day Santorini erupted and sent storms of fiery lava and pumice

raining throughout the ancient world. The surviving population, weakened by the natural disasters, soon succumbed to marauding and opportunistic hordes.

In the traditional history scholars agree that king Suppiluliumas II was the last king of the Hittite Empire. Little is known about his reign, though one letter from Ugarit, found in the kiln of the destroyed city, suggests that famine plagued the Hatti land for much of his reign. Does this imply that Hittite crops had been recently destroyed? It is of some interest that in his brief reign Suppiluliumas II was compelled to fight off invaders coming to the southern shores of his kingdom from Alashiya (Cyprus). In the words of Johannes Leymann:

> The Sea People, this time genuinely sea-borne, had by now reached and occupied Alashiya (Cyprus) on their way to Syria. Suppiluliuma made a last desperate attempt to recapture the island. 'I mustered ... and swiftly reached the sea - I, Suppiluliuma the Great King. But ships from Alashiya opposed me three times in battle in the midst of the sea. I destroyed them. I seized the ships and set them ablaze in the midst of the sea. But when I came on to dry land, the foes from Alashiya opposed me in battle. *The Hittites* 296

According to Leymann "We do not know the sequel".

At least one other hint that these were troublesome times is found in a sequence of oaths of loyalty, recorded on tablets, required of multiple Hittite subjects. Such oaths were common in treaties with foreign kings. They are otherwise unknown among the rank and file. Once again we follow Leymann:

> Thanks to a discovery made in 1953, we know that the last Hittite king was another Suppiluliuma ... A brother of Arnuwanda, he reigned ca. 1200 during a period of such turmoil and dissolution that he was obliged to make his subjects swear an oath of allegiance in order to assert his authority. The following declaration was made by a senior scribe on wooden tablets at the Hittite court: "I shall defend the issue of my lord Suppiluliuma alone. I shall not support another man, descendant of Suppiluliuma the First, descendant of Mursili, descendant of Muwatalli and Tudhaliyas' This is only one of several such oaths of allegiance, most of them cursorily inscribed. *The Hittites* 295

We hear no more from Suppiluliuma. The Hittite Empire is silent. It is sometime around the year 760 B.C.

Ugarit

The ancient city of Ugarit (modern Ras Shamra) was situated less than 100 km from the southern border of the Hatti land. Throughout much of the Hittite Empire period it headed a vassal state, bound by treaty to the Hittites. It is not surprising that its end coincided with that of its parent. Evidence from the ruins of Ugarit suggests that it fell at the hands of the same rebels who dealt the killing blows to the Hatti land. Margaret Drower provides the consensus view of the end of the city in her *Cambridge Ancient History* article on the topic. She quickly glosses over the reigns of the predecessors of Amurappi, the last king of the city. Little is known about them.

> Ibiranu was a contemporary of Tudkhaliash IV and probably also of his successor Arnuwandash III. The next king of Ugarit, Ibiranu's son Niqmaddu III, can have had only a brief reign; whether 'Ammurapi, who followed him, was of the royal line or no is uncertain, for, contrary to the usual custom, his parentage is nowhere mentioned; he is likely to have been of the same generation as his predecessor. CAH II.2 145

Several years after the reign of Amurappi began, Suppiluliumas II began his reign in Hatti. If we are correct, his coronation may have coincided with the eruption of the Thera volcano. Almost immediately, we see signs of insurrection in the kingdom. Troops are sent from Ugarit to assist the Hittites. Famine also grips the Hatti land. Famine apparently also threatens life in Alashiya, perhaps precipitating the naval migration from the island (in search of food?) referred to earlier. Ugarit was summoned to assist both the Hittites and the Alashiyans. Drower discusses the events.

> Shuppiluliumash II now ascended the Hittite throne and, facing a mounting tide of threatening disaster, found himself relying more and more on the fleet of his most important vassal on the Levant coast. The blow was not long delayed. In the ruins of the latest level of the palace at Ras Shamra, the kiln used for baking tablets was found to be packed full of documents, abatch of about one hundred brought by the scribes when freshly written; many are transcriptions into alphabetic Ugaritic of letters and despatches which must have been received in the weeks - even the days - before the fall of the city: there had been no time to take them from the kiln. The immediacy of the danger facing Ugarit is implicit in the wording and content of some of these and other tablets. The Hittite king asks urgently for a ship and a crew to transport grain from Mukish to the Hittite town

> of Ura in Cilicia, as a 'matter of life and death' since there is famine in the area. In making this demand, the Hittite refers to an act of liberation whereby he has formally released the king of Ugarit (probably 'Ammurapi) from vassalage, but he makes it clear that Ugarit has not been absolved from all her obligations towards her former overlord. Famine may also have afflicted Alashiya at this time: a certain Pagan whose letter to the King of Ugarit was one of those found in the kiln, calls the Ugaritian 'my son', perhaps indicating that a dynastic marriage linked their houses; he asks for a ship to be sent with food supplies for the island. In reply, 'Ammurapi informs his 'father', the king of Alashiya, that he has not a ship to spare, since the enemy has plundered his coasts, while his own fleet is in the Lukka lands and his troops in the land of the Hittites. CAH II.2 145-6

According to Drower "only one known situation fits this predicament: the approach of the 'Peoples of the Sea' whose destructive progress by way of Qode (Kizzuwadna), the Khatti-land, Carchemish, Alashiya and Amurru is all too briefly related by Ramesses III in his inscription on the north wall of the temple of Medinet Habu." (op. cit. 146)

We disagree. The Medinet Habu text to which Drower refers, the same text which caused Johannes Leymann to identify this same group of "Sea Peoples" as the cause of the Hittite collapse described above, belongs to a later phase of the social disruption caused by the eruption of the Santorini volcano. In the revised history we have dated the beginning of the reign of Ramses III around the year 757 B.C.. His fifth year, in which these particular sea-peoples invaded Egypt for the first time, must be dated around 752 B.C., about a decade after the dual problems of famine and insurrection mentioned in the Ugaritic documents. We will return to this subject momentarily when we discuss the Egyptian situation.

Then who are the enemy who threaten the Hatti land and are plundering the coastline around Ugarit? The answer is suggested by the historical context in which we have placed the event, one in which a widespread devastation of crops has caused panic and looting. Most of the rebels are probably local. Others may originate from neighboring states where famine has prompted an exodus in search of food. The situation in Ugaritic described by Drower raises many questions in support of this thesis. Why is an island nation like Alashiya unable to muster a single ship to feed its population? And why is a port city like Ugarit unable to supply ships in response to urgent requests for help? Granted that its military fleet (or what was left of it) was assisting elsewhere; surely a

thriving port city such as Ugarit harboured hundreds of commercial vessels at any one time. Where were they? And how did a shortage of food occur so suddenly and simultaneously both in the Hatti land and on Alashiya? The answer is provided by the revised history of the period in question.

When Santorini erupted in 765 B.C. vast tidal waves must certainly have overwhelmed both Alashiya and Ugarit. Not only were ships destroyed overnight, but stored food supplies as well. Ugarit had recourse to inland resources to resupply its population. Alashiya did not. Additionally, both the island and the city, indeed all of Anatolia, were suddenly overwhelmed with fiery ash hailing from the heavens. Earthquakes toppled walls, building and crops alike were destroyed by fire, a layer of ash blanketed the ground. We assume that the letters in the ovens of Ugarit were written less than a year following the Thera explosion. In the interim peoples throughout the Near East scrambled frantically to obtain food. What they could not obtain by negotiation they attempted to attain by force. We assume that the enemy that was confronted in the Lukka lands by Suppiluliumas was local, his army acting to suppress acts of civil disobedience on the part of the local population, or pillage by neighboring peoples. It is significant that the texts do not name the enemy.

We also assume that the texts from Ugarit predate the one mentioned by Leymann earlier, that which speaks of a conflict between ships controlled by Suppiluliumas (probably originating from Ugarit) and those acting on behalf of Alashiya. This later conflict assumes that Alashiya has by this time obtained a few ships and is attempting to pillage supplies from the Anatolian coastline. It is a battle for survival. Both nations are desperately in need of food.

Around this same time, Ugarit itself is ransacked and plundered. There is at least one letter that suggests that the vandals may have come from the east, through Cilicia, and attacks Ugarit from the north. We quote Drower once again.

> At the approach of the enemy, Shuppiluliumash must have summoned his vassals in North Syria to his aid, and Ugarit, loyal to the last, must have sent her whole army. One of the letters found in the kiln appears to be an urgent dispatch sent to

> the king in Ugarit from the commander of the army in Lawasanda (Lawazantiya) in Cilicia, which his troops had fortified in anticipation of attack. The enemy is nowhere mentioned by name, probably because so motley a horde had no collective name. Their presence in Mukish only a few dozen miles from Ugaritian territory, is indicated in a letter of Ewir-Sharrum, another of the Ugaritian generals in the field, to the queen or queen-mother, in the absence of the king at the front. Part of the letter is unfortunately damaged, but it sounds the note of extreme urgency and makes a reference to Mount Amanus, though a contingent of two thousand horses (equivalent to a thousand chariots, a very formidable force) is apparently still at the king's disposal. Other letters which may well date from this time of crisis tell of looting and burning. CAD II.2 146

Drower is careful to point out later in her article that there are "many obscurities" in this letter from the general; "moreover in script and language it differs from the other tablets in the archive and its date is therefore problematical". We should therefore not read too much into it. All we know for certain is that Ugarit ultimately succumbed to unnamed marauders. Already severely damaged by earthquake and fire the city was ransacked by opportunists in search of food and fortune. Its demise may be dated less than a year after the Santorini eruption. Small wonder that archaeologists are divided on the cause of its downfall. Here the earthquake and fire, and subsequent ransacking, came in such quick succession that scholars are unable to tell which preceded and which followed. Schaeffer, the first to excavate the city, was confused.

> Of the anxiety of the king and people of Ugarit in the face of impending danger the tablets leave us in no doubt. Whether or not the destruction of the city was due to enemy action is less certain. M. Schaeffer, the excavator of Ras Shamra over more than forty years, who long held the view that the Peoples of the Sea were responsible for the final pillage and burning of Ugarit, has now reached a different conclusion. Ugarit, he suggests, may have come to terms with the invaders and persuaded them to bypass the city. CAH II.2 147

We understand the dilemna confronting Schaeffer. There is overwhelming evidence that earthquake and fire destroyed the city, covering it with a thick layer of ash. There is also evidence that marauders ransacked, and perhaps burned the city. Which event came first? We believe that Schaeffer should have maintained his original position. It is the historical context, not the archaeology, that informs our conclusion. That context was denied to Schaeffer by the errant

chronology of the traditional history. Ugarit was partially destroyed in 765 B.C. as collateral damage of the great Santorini eruption. It was ransacked and looted perhaps only months, certainly less than a year later, as famine and frenzy gripped the population of the entire near east. No wonder there is confusion.

Syria/Israel/Judah

There exists a secure link between the end of the Hittite Empire, the ultimate destruction of Ugarit, and the mass movement of migrant peoples in the final days of the 19th Egyptian dynasty and the early years of the 20th. When we lowered the dates for the Egyptian 19th dynasty by 450 years, the dates for Hatti and Ugarit necessarily moved in tandem. It follows that our argument thus far, with the exception of the late date, might ultimately appeal to scholars. There is no fundamental reason why the migrations of "sea peoples" or the activity of vandals in the last days of Ugarit could not have resulted from a disruption in the food supply consequent to a massive eruption of some volcano in the Mediterranean. Nor is there any great contradition in identifying the cause as the Santorini explosion. What scholars will despute most vehemently is the 8th century date we have assigned to the event. It is for this reason that our discussion of happenings in Syria, Palestine and Assyria are critical aspects of our argument. Our revision has left the chronologies of these three countries intact. It is therefore of paramount importance that we find evidence that a massive explosion of the Santorini variety, accompanied by tandem destructions by earthquake and fire such as we have observed elsewhere, was felt along the eastern Mediterranean coastline. It is also vital that the event can be dated with some certainty around the year 765 B.C. We hope to find confirmation that the coastline was subject to gigantic tidal waves, that the land suffered a deluge of volcanic molten rock and that the atmosphere was polluted with ash, obscuring the sun for a considerable time. There should be evidence of mass starvation and social unrest. If not, there is something amiss in our chronological reconstruction.

We are not disappointed.

The Great Raash

Around the year 765 B.C., during the reign of Uzziah king of Judah, the eastern Mediterranean experienced the trauma of what the Hebrew Bible calls "a great raash", generally translated earthquake, but with much broader meaning. Upheaval or disruption might be a better translation. The Jewish prophet Amos devotes an entire prophetic book to a detailed summary of what transpired in Syria. The prestigious *International Critical Commentary* dates the ministry of this prophet to the years 765-750 B.C. That is only an estimate. We assume it began several years earlier. The vision recorded in the *Book of Amos* in the Hebrew Bible must be dated to the beginning of this propohet's ministry. This important book has been the subject of much scrutiny. It deserves yet another look.

The introductory byline of the *Book of Amos* sets the stage:

> The words of Amos, who was among the sheepherders from Tekoa, which he envisioned in visions concerning Israel in the days of Uzziah king of Judah, and in the days of Jeroboam son of Joash, king of Israel, two years before the earthquake (raash). (1:1)

Amos' reference to the *raash* does not function merely to date his prophetic word. It is rather an announcement of his intention to describe the event. What follows is a panoramic vision of the *raash* as it devastated the key centres of population from Damascus to the Gaza strip, its arrival announced with a thunderous roar in Jerusalem (1:2). From north to south the identical scene played out, fire (from heaven) consuming the citadels of Damascus, bringing to an end the royal line of Hazael and Ben-Hadad (1:3-5); and fire (from heaven) consuming the walls and citadels of Gaza, Ashdod, Ashkelon and Ekron (1:6,7) all but ending Philistine civilization (1:8). Elsewhere the scene is repeated. Tyre on the Meditteranean coast suffered the identical fate as Damascus and Philistia (1:9,10). Likewise Edom and Ammon further inland (1:14,15). This was no mere earthquake; it was rather a holocaust. We should not be deceived by the brevity of the description. Significant for our revision, these events are clearly dated around the year 765 B.C.

The description continues for nine chapters. All of the anticipated elements are present. The coastline shudders and the earth shakes, causing city walls and buildings to fall. A gigantic tidal wave or multiple waves vent their fury on the land, punishment, according to Amos, for the sins of the nations. By degrees the sky is blackened by molten ash and cinders. Fire fell from heaven, turning cities into raging infernos. All this was viewed as a judgment from the Lord for multiple national sins.

> He who made the Pleiades and Orion
> And changes deep darkness into morning
> Who also darkens day into night
> Who calls for the waters of the sea
> And pours them out on the surface of the earth
> The Lord is His name
> It is He who flashes forth with destruction upon the strong
> So that destruction comes upon the fortress (5:8,9; cf. 9:5-6)

Tidal waves came like a torrent, sweeping away even those most securely sheltered, depositing their remains on the highest elevations.

> You will go out through breaches in the walls
> Each one straight before her
> And you will be cast to Harmon, declares the Lord (4:3)

Ash swept in like a fog. The sun was obscured. Darkness and gloom prevailed, awesome and frightful.

> Will not the day of the Lord be darkness instead of light
> Even gloom with no brightness in it. (5:20)
> And it will come about in that day, declares the Lord God
> That I shall make the sun go down at noon
> And make the earth dark in broad daylight (8:9)

A great earthquake shocked the land; aftershocks continued, the land rippled like a wave.

> Because of this will not the land quake
> And everyone who dwells in it mourn
> Indeed, all of it will rise up like the Nile,
> And it will be tossed about
> And subside like the Nile of Egypt. (8:8)

Death and destruction were immediate and widespread. Property and crops were destroyed in minutes (5:3,11). Entire cities were all but exterminated. Ninety percent of the population died.

> For thus says the Lord God
> The city which goes forth a thousand strong
> Will have a hundred left
> And the one which goes forth a hundred strong
> Will have ten left to the house of Israel. (5:3)

Scholars have been confused by the language of Amos, perplexed at how to explain the absolute devastation he describes. Most interpreters view the entire book as a metaphor, believing that the devastation thus described was the result of warfare. They point to the fact that twenty years after the prophecy the Assyrian armies of Tiglath Pilezer III did in fact invade the Levant, causing indeterminate damage and inflicting innumerable casualties. There are in fact several indications in the text that warfare did accompany the events described elsewhere.

> Therefore, thus says the Lord God
> An enemy, even one surrounding the land
> Will pull down your strength from you
> And your citadels will be looted (3:11)

We argue instead that the few references to war are expected. From the experience of Hatti and Ugarit we anticipate precisely this secondary destruction by rebels/vandal/opportunists. In Syria the house of Ben-Hadad and Hazael ended. A contest for the vacant throne must surely follow. Widespread looting of destroyed cities is predictable. And as we will observe in our discussion of Egypt, a decade after the destruction of the Levant in 765 B.C. Sea-Peoples did invade Syria en mass, and sojourn there en-route to Egypt. But neither these migrant armies, nor the Assyrians later, are known to have wrought havoc even remotely approaching what Amos envisions. The prophet is most certainly attributing the destruction to natural causes and the looting to human intervention following. Any other interpretation is strained.

A half century ago Immanuel Velikovsky recognized that the language of Amos should be taken literally. In his *Worlds in Collision* he attributed a cosmic cause to the earthquake, the tidal waves and the fire from

heaven. But there is no hint in the prophetic writing of Amos of this event is of extra-terrestrial origin. And the date 765 B.C. convinces us that our interpretation is correct.

The great *raash* in Uzziah's day was remembered for centuries. It became the unique symbol of a time when the judgment of God was announced to the world with great natural accompanying wonders, most importantly the pollution of the heavens with clouds of ash and cinders which block out the sun, turning day into darkness. Thus declares the prophet Zechariah speaking hundreds of years later:

> And you will flee by the valley of My mountain, for the valley of the mountains will reach to Azel; yes, you will flee out as you fled before the *raash* in the days of Uzziah king of Judah. Then the Lord, my God, will come and all the holy ones with Him. And it will come about in that day that there will be no light; the luminaries will dwindle. For it will be a unique day known to the Lord ... Zech 14:5-7

The pollution of the atmosphere was without doubt the most vivid and lasting effect of the Santorini eruption. It must have persisted for months, if not years. And its effects must have been felt around the world. If so, then we should expect to see some reference to it in the vast archives of the Assyrians, apart from the Hebrew Bible the best preserved source of knowledge about the Near Eastern world of the 8th century B.C. We should therefore turn our attention eastward to Assyria. But first one final question related to the situation which prevailed in Syria following the great *raash*.

Rezin

For well over a hundred years prior to the great *raash* Syria was ruled by a succession of kings bearing the dynastic names Ben-Hadad and Hazael. Amos is unequivocal is declaring an end to this succession around 765 B.C. Unfortunately there is no explicit biblical reference to the political situation which prevailed in the decades which immediately followed the *raash*. The next we hear from Syria the country is ruled by an otherwise unknown king named Rezin.

We first encounter Rezin decades later, around the year 745 B.C. At the time he has allied himself with Pekah, king of Judah, and is proceeding to attack Jerusalem (2 Kings 16:5,6). Ahaz, the Judaean king, responded by seeking the help of the Assyrians, now ruled by the infamous Tiglath-Pilezer III. As a result "the king of Assyria listened to him; and the king of Assyria went up against Damascus and captured it, and carried it away into exile to Kir, and put Rezin to death" (2 Kings 16:9).

We mention Rezin in order to highlight one interesting aspect of his reign. Our only information regarding him in the Jewish historical literature describes his violent end. He is portrayed in this brief vignette as an aggressive military leader. Nothing more is said about him. We believe his reign began twenty years earlier, in succession to the dynasty of Ben-Hadad and Hazael. And we believe that very soon following the *raash* he led an army, perhaps including foreign mercenaries, to successfully attack Egypt, weakened by the same upheaval that decimated the Levant. Surprising as it might seem, for a brief moment in time this Syrian king became an Egyptian pharaoh. But here we are getting ahead of ourselves. We will take up that story in a moment.

Assyria

It is estimated that when Santorini exploded "about 7 cubic miles (30 cubic km) of rhyodacite magma was erupted" and that "the plinian column during the initial phase of the eruption was about 23 miles (36 km) high" (see volcano.oregonstate.edu/vwdocs/volc.../santorini.html.) Much of that material must have remained in the atmosphere for years, blocking out the sun around the world. The heavier molten ash would have dissipated in the first few days following the explosion, and depending on wind conditions would have spent its fury within a radius of three or four hundred miles. The eastern shores of the Mediterranean might have been its limit.

Assyria lies five hundred miles further east, and was spared the fury of the fiery downfall. But the ash remained. The sun was obscured. Day turned to night as it did in Amos's world. And the event was duly noted.

As a rule the Assyrian annals are not interested in astronomical events. Lists of kings and their exploits were produced in abundance, citing fact upon fact in concise and monotonous sequence. But in the most inoccuous of places and in the most matter of fact manner, one scribe at one moment in time made an exception to the rule, and remarked on the fact that the light of the sun was obscured. His remark was recorded around 765 B.C. Unfortunately for history, Assyriologists have misconstrued his remarks.

One of the most important sources for the determination of Assyrian dynastic history is the Eponym Canon, a document which itemizes, year by year, the reigns of the rulers of the country over two centuries, associating each year with some notable person (eponym) or event. Its relative chronology is extremely precise, affording scholars the opportunity to establish with precision the sequence of Assyrian kings together with their reign lengths for much of the 9th and 8th centuries B.C. And since several of these Assyrian kings interacted with the kings of Syria and Israel, it is possible to link Assyrian and Syrian chronologies with that of Israel with some degree of accuracy. By comparing the biblical and Assyrian data it can be determined with some certainty that the reign of the Assyrian king Ashur-dan III should be dated roughly in the years 773-755. The Santorini explosion of 665 B.C. must therefore have taken place approximately a decade into his reign.

Hugo Winckler, one of the most notable Assyriologists from the pioneering days of this discipline, wrote his *History of Babylonia and Assyria* early in the 20th century, following closely the data provided by the Eponym Canon. Concerning the reign of Ashur-dan III (773-764?) he writes:

> He marched three times into Syria, the first time against Damascus, and the second against Chata-rikka to the north of it. Twice he advanced into Babylonia, in 771 and 767, where he sought to oppose the Chaldeans. The second half of his reign witnessed a weakening of his kingdom which compelled concentration of effort upon the maintenance of that which had been slowly accomplished in the tributary states. In 763 an insurrection broke out which, in the years that followed, was repeated in different quarters until by degrees a large part of the kingdom was involved. The Eponym Canon puts a division line before this year (the year which it tells us the eclipse of the sun occurred - a valuable notice for the determination of the old chronology) as it does before the beginning of a new

> reign; for, since the insurrection took place in Ashur, a rival king must have been called forth.... The Eponym Canon does not name the king who was raised to the throne by the insurrection, but from various statements it is clear that he was recognized as king. He was Adad-Nirari IV (763-755). HBA 234-5.

Several aspects of Winkler's summary of the reign of Ashur-dan require explanation. We begin by reminding readers well versed in ancient history that the reference to the obstruction of the sun noted in the Eponym Canon in the 10th year of Ashur-Dan has from the outset of Assyriological studies been interpreted as a solar eclipse. And since comparison with Jewish historical literature places Ashur-dan "roughly" in the third to fifth decades of the 8th century, scholars from the outset sought to determine the date of the event by astronomical means. They were immediately successful. The fact that a solar eclipse took place in the near east in 763 B.C. clinched the identification, and with the Eponymn Canon in hand Assyrian chronology was rigidly secured in its present position before the end of the 19th century. The reign of Ashur-dan must have begun in 773 B.C. and it ended, according to the Canon, eighteen years later, in 755 B.C. But this interpretation of the "obliterated sun", and the results which issued from it, should be seriously questioned.

Several challenges should be issued to Winckler's remarks, quoted above. In the first place the Assyrian text merely states that in the eponym year in question the "the sun did not shine". The reference is to an event of considerably more importance than a momentary darkening of the sun, an eclipse lasting for at most a few hours. The Assyrians were not a backward people, awestruck by an unexpected and terrifying celestial event. Assyrian astronomers were well aware of eclipses of both moon and sun and able to predict their arrival with some degree of accuracy. The eclipse of the sun would hardly deserve mention in a document concerned to uniquely identify the regnal year of a king. Rather, we suspect, this was the year that day turned to darkness, the beginning of a prolonged period of dusk and gloom that lasted for months, and perhaps for years. There is evidence in the Canon itself that we are right.

Winckler notes the fact that before mentioning the year in which the sun ceased shining "the Eponym Canon puts a division line", something it is

careful to do elsewhere only at the end of the reign of an Assyrian king, the end of an era so to speak. The presence of a line before the 10th year of Ashur-dan is unexpected and unprecedented, since his reign continued for another decade. Winkler attempts to explain its presence by inventing and inserting at this stage his king Adad-Nirari IV and assigning to this king the years 763-755 B.C., thus ignoring the plain witness of the Canon. We understand his reasoning. Something extremely important must have begun in the 10th year of Ashur-dan III. But what? We suspect, though admittedly we cannot prove, that there began this year a period of prolonged darkness, unprecedented and unexplained. We believe it continued for years. And we date the event to 765 B.C., not 763 B.C. We should perhaps point out, in passing, that altering the date for the 10th year of Ashur-dan by several years implies that all chronologies for near eastern civilizations which are referenced to Assyrian chronology are off by the identical two years, since this date was pivotal in assigning an absolute chronology to the Assyrian kings. This means, in turn, that every 1st millenium date used in our revision to this point in time would need to be moved backward two years, to agree with a revised Assyrian chronology. Needless to say we have no intention of turning back the clock. There remains some doubt that our interpretation of the Eponym Canon is correct, and our date for the Santorini explosion was only approximate. What is noteworthy here is the fact that the Canon reference to an obscured sun provides a possible parallel to events taking place elsewhere in the near east around the identical time. And as coincidences multiply, certainty grows.

We should also note in passing one further curiosity. Not only does a new era begin in the 10th year of Ashur-dan, whether 765 B.C. or 763 B.C. in absolute terms, but there is clear evidence that this era ended around the year 747 B.C. when a new age began. Many Assyrian documents are referenced to this new age, well known to scholars as the "age of Nabonidus" Why it began and what were its characteristics has never been determined. Immanuel Velikovsky makes the following insightful comments:

> In -747 a new calendar was introduced in the Middle East, and that year is known as "the beginning of the era of Nabonidus." It is asserted that some astronomical event gave birth to this new calendar, but the nature of the event is not known. The beginning of the era of Nabonassar, otherwise an obscure

> Babylonian king, was an astronomical date used as late as the second Christian century by the great mathematician and astronomer of the Alexandrian school, Ptolemy, and also by other scholars. It was employed as a point of departure of ancient astronomical tables. WC 210

Quoting from Cumont's *Astrology and Religion among the Greeks and Romans*, Velikovsky continues:

> "This was not a political or religious era ... Farther back there was no certainty in regard to the calculation of time. It is from that moment that the records of eclipses begin which Ptolemy used." (Cumont 8,9) What was the astronomical event that closed the previous era and gave birth to a new era? WC 210

Velikovsky corrects concludes that the great *raash* of Uzziah's day must have been the ultimate cause. But he wrongly concludes that the *raash* happened in 747 B.C. We maintain that the sun was first obstructed in 765 B.C. And when the sun was obstructed so also was the nighttime sky. Observation of the movements of sun and moon and stars which guided the calculations of the Assyrian astronomers were precluded. In a very real sense the Assyrians lost the ability to precisely regulate their calendar. If we are correct the sky did not clear sufficiently to resume calendrical determinations for 18 years! Of this we cannot be certain, but the fact that the record of eclipses used by Ptolemy begins only in 747 B.C. agrees with our suggestion.

It is time to turn our attention to Egypt, whence began this lengthy and circuitous excursus. We have come full circle.

Egypt

In our second chapter we outlined the histories of the 18th and 19th Egyptian dynasties, based to a large extent on the chronology represented by the Berlin Genealogy. To Ramses II and Merenptah, the two most notable kings ruling at the end of the 19th dynasty, we assigned the dates 840-774 and 774-764 B.C. respectively. We also said concerning the ephemeral kings Seti II, Amenmesse, Siptah and Twosre, that they almost certainly did not rule in succession at the end of the dynasty, as the traditional history would lead us to believe. In all likelihood their reigns

overlapped the terminal years of Merenptah, as all of them contested simultaneously for the throne.

We repeat our earlier claim that most of the (few) monuments of this period are undated and there is no clear evidence that these kings ruled in succession. In fact, the evidence suggests otherwise. The four terminal kings appear to have been confined to the vicinity of Thebes, where they exercised some limited political power till around 759 B.C., the date we assign to the beginning of the reign of Setnakht (759-757 B.C.), the patriarch of the 20th dynasty kings. These dates allow for the fact, expressly stated by Ramses III, that an interregnum existed in Egypt prior to the advent of his father Setnakht. To Ramses III, by far the most prominent king of the 20th dynasty, we have previously assigned the provisional dates 757-725 B.C.

The confusing evidence attesting the brief reigns of the four ephemeral kings is reflective of the chaotic conditions which prevailed in Egypt both prior to and immediately following the great raash of 765 B.C. The country was threatened from without for much of the reign of Merenptah through to the first decade of the reign of Ramses III. We believe that the series of eruptions culminating in the cataclysmic explosion of Santorini was a primary cause. There are only a few items from this time frame deserving of comment here. We begin with the reign of Merenptah.

Merenptah (774-764 B.C.)

From the 5th year of Merenptah (770 B.C.) well into the reign of Ramses III Egypt was threatened by marauding Sea-Peoples, though we must distinguish between the early and later phases of this series of aggressions. The early attacks were lauched by Lybia, accompanied by other Meditteranean peoples including the Ekwesh, Teresh, Luka, Sherden, Shekelesh, identified only as "northerners coming from all lands." It is interesting to note that the lands involved encircle Santorini like a wreath. Breasted, who has published all the relevant inscriptions from the reign of Merenptah, gives his impressions of the lands of origin of the aggressors.

> Since the study of Sardinian art by Perrot and Chipiez, as Muller has shown, we must accept the Sherden as Sardinians; the Teresh may then equally well be the Etruscans (Tyrsenoi), and the Shekelesh might be the Sikeli (if "sh" be an ethnic termination in these western names; ...) Maspero has suggested Sagalassos in Asia Minor. The Ekwesh are not impossibly the Achaeans, and from Asia Minor are the Luka or Lycians. BAR III fn. a, 239

It is not necessary to assume that this incursion of foreigners into Egypt was prompted by starvation in or desolation of the respective home countries of these peoples, nor that the desolation resulted from volcanic activity at Santorini. But we do consider that source to be the likely cause. The year is 770 B.C., only five years before the great explosion. We have previously argued that there did exist a lengthy period of volcanism on the island preceding the great eruption of 765 B.C. The coincidence of time and geography is compelling. At least one reference in the Merenptah archives indirectly supports this conclusion.

It is at least interesting to observe that Merenptah, in his Great Karnak Inscription, in the same breath in which he mentions with scorn the invading Libyan chief, remarks on how he had recently sent "grain in ships, to keep alive that land of Kheta." This action is most naturally connected to the famine which plagued the Hittites under Suppiluliumas II. It follows therefore that it should be attributed to the same cause, which we have identified as the volcanism at Santorini. Breasted is convinced that Merenptah is here blaming the Hittites for complicity in the recent attacks, thus accusing them being ingrates as well as aggressors. Considering that the Luka were allied with the invaders, and that the Luka were a tributary nation of the Hittites during the late Empire period, he is probably correct.

We move quickly from the beginning to the end of the series of invasions of Sea-Peoples, i.e., those which plagued Egypt in the early days of Ramses III.

Ramses III

Ramses III ruled, according to our revised chronology, from 757-725 B.C. In his 5th, 8th and 11th years he faced three separate hordes of invading nations, the first led by Libya, assisted by the Philistines (Peleset) and the Thekel (Sicilians?); the second by a northern confederacy "disturbed" from their isles, who are said to have set up camp in Hatti and Syria, en route to Egypt; and the third by another Libyan confederacy. It would be of some benefit to discuss these invasions in some detail, but the principal of marginal returns would soon set in. Rather, we focus our attention on the invasion of the 8th year, 750 B.C. if we have correctly dated Ramses III.

It is generally agreed that all of these invasions were provoked by a common cause, some disaster in the countries of origin of the invading peoples. Something or someone "disturbed" these confederates "in their isles", this according to the Medinet Habu inscriptions in which Ramses has recorded the events. In the case of the 750 B.C. invasion we read:

> The countries - - , the [Northerners] in their isles were disturbed, taken away in the [fray] - at one time. Not one stood before their hands, from Kheta, Kode, Carchemish, Arvad, Alasa, they were wasted. They set up a camp in one place in Amor. They desolated his people and his land like that which is not. They came with fire prepared before them, forward to Egypt. Their main support was Peleset, Thekel, Shekelesh, Denyen, and Weshesh. (These) lands were united, and they laid their hands upon the land as far as the Circle of the Earth. Their hearts were confident, full of their plans. BAR IV 37-8

This invasion, according to the inscription, arrived both by land and by sea, as the invading hordes consolidated their strength in Anatolia (Kheta and Kode), moved on to Syria (Carchemish, Arvad and Amor) where they subdued what remained of the northern and southern regions of the country, then on to Egypt. We let Breasted supply the details of what follows in the inscription. He begins by linking this invasion with that which preceded it three years earlier:

> Already in Ramses III's fifth year the tribes of the southern coast of Asia Minor and the maritime peoples of the Aegean had sent some of their advanced galleys to assist the Libyans in their war of that year against Egypt. Or, as in Merneptah's day, the plundering crews of their southernmost advance had incidentally joined

> the Libyan invasion. These were but the premonitory skirmishing-line of a more serious and more general movement. The peoples involved were probably Cretan Peleset, a settlement of whom later became the biblical Philistines; the Thekel, who may be the Sikeli, later of Sicily; the Shekelesh, the Denyen or Danaoi, and the Weshwesh, who are of uncertain origin. [A footnote informs us that the papyrus Harris adds the Sherden, probably from Sardinia, to the list of invaders]. Owing to pressure from uncertain sources without, large numbers of these peoples, accompanied by their wives, children, and belongings, in clumsy ox carts, left their homes, and moving eastward along the coast of Asia Minor, penetrated Syria. They were accompanied by a strong fleet also. In the author's opinion, this movement was really a "*Volkerwanderung*," not merely an invasion, with a few families of the chiefs. They were strong enough to hold all northern Syria at their mercy; from Carchemish, through the Syrian Hittite conquests to the coast, as far south as Arvad, and inland as far south as Amor, they plundered the country. They had a central camp somewhere in Amor. BAR IV 33-4

Details of the actual invasion of Egypt by this motley horde are of little interest to this revision. We are concerned only to point out the remarkable agreement with our proposed reconstruction of the history of the period. Mainstream scholars are at a loss to explain what is happening. Many questions are left unanswered. What tragic event could possibly explain the mass displacement of peoples from remote geographic regions of the eastern Mediterranean, not at a single moment in history, but at various times spanning at least a twenty year time frame (much longer in the traditional history)? How do we explain how the northerners in the 8th year of Ramses III could encamp in the vicinity of Kheta and Kode and Carchemish and encounter virtually no opposition? Why is Kheta used here as a place name only? Where are the Hittite people? Why are the Philistines involved in this supposedly 13th century event, a nation that first appears in the Hebrew Bible in the days of kings David and Solomon in the 10th century B.C.? And why do we find mention of the Danaanians, occupants of southern Anatolia named in 8th century documents and otherwise known only from oblique references in Homers epic war story. The reader will recall that we mentioned them in an earlier chapter in connection with Suppiluliumas II. Finally, we wonder why the Etruscans are mentioned if this is the 13th century B.C.? Scholars have argued for centuries that this group of European immigrants, ancestors of the Romans, first arrived in Italy in the 8th century.

We have already provided our answer to these problems. The events in question belong to the 8th century, not the 13th. Already in 770 B.C. eruptions of Santorini were causing havoc in the Mediterranean basin. Many Jewish prophets besides Amos, including Isaiah, Micah and Haggai, were well aware of the disasters already experienced by the Mediterranean world. Their prophecies of future catastophes in part reflect their knowledge of present conditions. Peoples around the Mediterranean had already sought shelter or fled local devastations. The great disruption of 765 B.C. only exacerbated the problem. The damage was monumental. Cloud enshrouded the region for decades; aftershocks and secondary eruptions perpetuated the fear. Drought persisted. Famine caused vast segments of the surviving peoples to migrate and pillage in search of food. As late as 750 B.C., the 8th year of Ramses III, the gloom persisted. Another three years and the skies had opened sufficiently to permit some degree of normalcy to life in the Mediterranean. The Santorini disaster had run its course. In Assyria a new age began. In Egypt the Libyans tried one more time to find habitable domains but by now the Europeans were content to remain in their homelands and rebuild. It was 747 B.C.!

In time Santorini was forgotten. Or was it? Some have argued, in spite of maintaining a second millenium date for the event, that the destruction of the island persisted in the myth of Atlantis, still remembered by Plato in the 4th century B.C. The theory may be correct in essence, though wrong in chronology. But that is a story for another time.

We have but one final observation and with that this segment of our revision ends. If multiple peoples of the near east were dislodged from their homelands as early as 770 B.C., the 5th year of Merenptah, and continued to seek refuge through 750 B.C., the 8th year of Ramses III, then surely there must have been some intrusion of foreigners into Egypt in the years between these dates, particularly in the immediate aftermath of the 765 B.C. explosion. And since Egypt was at this time "ruled" by an ailing king Merenptah, and a succession of weak pharaohs contesting to succeed him, all ill equipped to ward off armed intrusion, we expect that at least the northern regions of the country were overrun during this time. We do not expect to find corroborative evidence of this occupation in the Egyptian monuments left by these rulers, whose preoccupation was

more with survival than posterity. We have dated the death of Merenptah a year after the Santorini outburst. He may have died fighting off the aggressors. In any case he was old and ineffective, having come to power late in life due to the extemely long life of his father Ramses II. The monuments suggest he was busy preparing his tomb for the afterlife. His would be successors were confined to the south of the country. The few inscriptions that remain from this period suggest that times were chaotic, but fail to document the fact that the country was overrun by foreigners. Fortunately one document composed in the immediate aftermath of the occupation survives to tell us what happened. It is more than enough.

The document in question is the so-called Papyrus Harris, "found by the natives at Thebes in 1855". Breasted describes the great papyrus:

> This remarkable manuscript is the largest papyrus extant, being no less than 133 feet long, and containing 117 columns, usually of twelve or thirteen lines. Written in a magnificent hand, it is the most sumptuous manuscript left us by ancient Egypt. The content of the document is not less remarkable than its external form. It is a detailed statement of Ramses III's benefactions to gods and men during his entire reign of over thirty-one years. It was compiled at his death by his son, to be placed in the king's tomb, and is distinctly mortuary in its character and purpose. BAR IV 88

As Breasted notes elsewhere, "the closing section, which is a short historical account of Ramses III's reign, has received much attention". In particular it offers us tantalizing detail concerning the interim between the end of the 19th dynasty and the beginning of the 20th. It tells us precisely what we have otherwise anticipated, that the country was in fact successfully overrun by foreigners shortly after the death of Merenptah.

According to the Harris Papyri, when Ramses III came to power ...

> "the Libyans and the Meshwesh were dwelling in Egypt, having plundered the cities of the western shore, from Memphis to Kerben. They had reached the great river on both its banks. They it was who plundered the cities of Egwowe during very many years, while they were in Egypt. Behold I destroyed them, slain at one time. BAR IV 201-02

This revelation that the 20th dynasty was preceded by a time of foreign occupation is stated even more explicitly in the introductory paragraphs

of the historical section which precedes this description. We quote Breasted one last time. In the inscription Ramses III is speaking:

> Hear ye, that I may inform you of my benefactions which I did while I was king of the people. The land of Egypt was overthrown from without, and every man was (thrown out) of his right; they had no chief mouth for many years formerly until other times. The land of Egypt was in the hands of chiefs and of rulers of towns; one slew his neighbor, great and small. Other times having come after it, with empty years, Yarsu, a certain Syrian was with them as chief. He set the whole land tributary before him together, he united his companions and plundered their possessions. They made the gods like men, and no offerings were presented in temples. BAR IV 198-99.

When scholars first read the papyrus they were dumbfounded by the revelation that Egypt had been overrun by foreigners prior to the beginning of the reign of Ramses III. Particularly disturbing was the information that a certain Yarsu, or Arsu, a Syrian chief, was among the intruders, and that this foreigner had imposed his rule on some portion of the country over several years. No historical antecedents could be found in the 12th century to explain the event. No explanation of what permitted this massive intrusion of a foreign element into Egypt was readily at hand. Scholars were unable to determine if this incursion brought the 19th dynasty to an end, nor for how long the invaders ruled the country. For the traditional history the entire episode is an enigma.

The revised history is more fortunate. Our date for the beginning of the 20th dynasty lies only six years following a massive volcanic eruption which devastated multitudes of countries in the eastern Mediterranean, Egypt included, bringing to an end the 19th dynasty, and opening the country to the invasion of multitudes of opportunistic refugees. Not only are we not surprised by the revelations of the Harris papyrus, but had that document not been preserved to confirm our expectations, we would have insisted that a foreign invasion of Egypt must have taken place in the years immediately following the Santorini explosion, bracketed by those that had occurred five years earliers and those that followed a decade later. But this time there was no strong dynastic leader to resist it.

And it is our good fortune to be able to claim, that at precisely that moment in history, Syria is ruled by an opportunistic and aggressive

chieftain named Resin, of which Arsu is but an Egyptian variant. Coincidence? We think not.

And on that note this segment of our revision ends.